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NADEL ON THE AIMS AND METHODS OF SOCIAL ANTHROPOLOGY *

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Nearly every sociological thesis proposes a new method which, however, its author is careful not to apply, so that sociology is the science with the greatest number of methods and the least results.

Poincaré

I *Introduction*

IN this paper I discuss critically the late Professor Nadel's views on the aims and methods of social anthropology. These are to be found in his long and difficult book *The Foundations of Social Anthropology*.¹

Only two problems raised by Nadel will be discussed. (1) What are the aims of social anthropology? (2) What, in general, will a characteristic attempt to achieve these aims look like?

Nadel's answer to (1) is that social anthropology aims at: (a) describing and (b) explaining rational behaviour. He argues that there is such a close relationship between these two aims that they can be treated as one in the solution to (2), as follows. An explanation is to be effected by means of general descriptive laws, which are 'fit' or 'required' correlations reached by induction. He then argues that we explain the social behaviour of *individuals* by means of laws on non-social (i.e. psychological) levels. We explain social *institutions* in terms of laws specifying the ultimate purpose they serve in society.

* Received 19.ii.60. This paper is part of a thesis on the methodology of social anthropology which I am engaged in writing. Sections of an earlier draft of it were read to Professor K. R. Popper's seminar at the London School of Economics and Political Science in May 1959; it has benefited greatly from the discussion there. In addition I wish to thank the following people who read and discussed the MSS with me: Dr J. Agassi, Dr I. Lakatos, Mr D. Scheinfeld, Dr P. Stirling, and Mr J. W. N. Watkins.

¹ S. F. Nadel, *The Foundations of Social Anthropology*, London, 1951

Against this powerful position I shall try to maintain that the aim (and actual practice) of social anthropology, like that of any other science—natural or social—is explanatory, i.e. the solving of problems. In criticism of him it will be argued: first, that description and explanation are distinct, the former being a means, not an end; second, that individual social behaviour cannot be explained on any level other than the sociological level; third, that his characterisation of social laws is inadequate for explanation; and fourth, that the functional explanation he expounds explains too much.

Naturally both the criticisms made and the way they are made will be influenced by my own position, which is grounded in Popper's methodology of science. This need not put off those of other views; I only hope the paper provokes them to criticise its Popperian foundations.

2 *Exposition*

2.1 *Nadel's Problem Situation.* What Nadel seems to be doing in the *Foundations* is justifying his claim that social anthropology is a major social science. This is evident when, after declaring that he is writing about *method*, Nadel notes that in earlier days he might have titled his book: 'Prolegomena to the Study of Society: Being an Enquiry into the Nature of Sociological Knowledge'. Social Anthropology, then, is the Study of Society; and to enquire into the aims and methods of social anthropology is to enquire into the Nature of Sociological Knowledge itself. This reflects the view that social anthropology is an all-embracing social science. Social anthropologists often seem tacitly to imply that social anthropology legitimately subsumes under it: sociology, economics, political science, comparative religion, comparative jurisprudence and (in Nadel's case) psychology.

The non-anthropologist might phrase his puzzlement at these grand claims in the form of three questions about social anthropology (of which the first two are a breakdown of problem (1) of the second paragraph, while the third is equivalent to problem (2)): just what is it about; what does it aim to do; and how does it propose to do it? The answers to these questions will allow us to judge whether, in its aims and methods, social anthropology approximates to our conception of a science.

Nadel admits, a little ruefully, that works on the aims and methods of the social sciences are, if anything, already too plentiful. He cites

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the brutal comment of Poincaré which heads this paper, and is careful to show that his book on method stems from the treatment of a genuine problem, namely a certain 'lack of agreement' between what is said about method and what the method actually is, in social anthropology.

It can be argued that this lack of agreement arose during the historical development of social anthropology. Anthropology began when, soon after their discovery, people asked why contemporaneous native societies were so different from ours. The original hypothesis was: native societies are different from ours because they are more primitive; they are at a stage our own society passed through long ago. Here an auxiliary hypothesis to the effect that human societies go through a Darwinian evolution from 'primitive' to 'civilised' was introduced.

The original hypothesis could not really be falsified but, instead of looking for ways to test it, later anthropologists made these criticisms: (a) such a hypothesis was not gained by scientific methods—careful observation, systematic generalisation, etc.; (b) no formulation of it explains all the known facts; (c) the problem is uninteresting anyway.

As a result there were modifications; a multi-linear model of the evolution of societies was substituted for the unilinear one. That so little was explained, even after this, was put down to (a); (a) in turn suggested that the problem was not well put. Interest then turned to a new problem: How do societies so very different from ours, so seemingly irrational, manage to function? Scientific methods were to be used on this problem. Particular cases were successfully explained, but nothing like a general or theoretical science resulted.

Nadel, a Baconian, blames this lack of results not on the methods but on failure properly to carry out the methods. The reasons for this being the intrinsic difficulty of the methods, and anthropologists' ignorance of them. Anthropologists are too wrapped-up in empirical problems to pay much attention to carefully analysing their aims and methods. Thus their (often polemical) methodological pronouncements, which are unavoidable in teaching, bear too little relation to what they are actually doing. There is a lack of agreement between the method anthropologists pay lip-service to, and what they actually *do*; that is to say, between how they *are* going about things and how they *say* they are going about things. He gives no examples, but a famous one is Radcliffe-Brown's brilliant preface to *African Systems*

of *Kinship and Marriage* where he strenuously denounces 'pseudo-historical conjecture', and later himself proposes some quasi-historical theories.

This sort of 'lack of agreement' is not an unusual state of affairs in science. Many working scientists are not at all good at describing their aims and methods. Nadel is interested in determining the extent of this 'lack of agreement' in social anthropology. At the same time he hopes this investigation will help to improve matters by 'bringing out into the open what other anthropologists have left unexpressed, rendering tacit methods explicit, and exhibiting their full import' (p. v). Nadel does not say *how* this process will help, but he implies that bringing out these tacit methods will facilitate criticism of them and perhaps lead thereby to improvement.

Nadel justifies his discussion of method on other grounds. He says that he is undertaking a systematic re-examination of 'things known' in social anthropology in the belief that this is a process upon which 'scientific progress rests in large measure'. From this it can be inferred that Nadel felt there was a certain lack of progress, a certain stagnation in the science of social anthropology. He rightly traces the cause of this to methodological shortcomings, and in particular to the problems of observation, description, classification, and explanation. About the first of these nothing will be said. The third—classification—so far as it is interesting, I take to be a part of description. The remaining two topics—explanation and description—are what will be discussed from now on; particularly the former. For I claim it can be shown that Nadel's examination of what is accepted as explanation in social anthropology, his incorrect analysis of it, and his consequent doubts about its adequacy, lead him to his questionable doctrine that there are (non-social) 'levels of explanation' possible in social science.

2.2 *The Subject Matter and Aims of Social Anthropology.* The aims and methods of social anthropology will be discussed in the form of the three questions posed early in section 2.1. 'What is social anthropology about?, i.e. what is its subject-matter?' 'What is it aimed to do with this subject-matter?' 'How is this aim to be achieved?', i.e. by what methods? Let us now take the first two questions.

Social anthropology, he says, works from a basis of 'statements on observation' towards the aim of describing and explaining social facts. But what, it will be asked, are these facts which are to be described and explained? They are, briefly, action and interaction between

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individuals; 'activity' here meaning aim-controlled or goal-oriented behaviour; that is, rational behaviour.¹

Nadel's designation of the aims of social anthropology as description and explanation neatly parallels a division of the subject into (descriptive) ethnography and (explanatory) comparative sociology, first made by Radcliffe-Brown. The two 'are facts and theory, a multitude of statements on particular observations and their explanatory synthesis' (p. 21). Nadel thinks this a little misleading, though, since it is undoubtedly the case that ethnography, even in the way its descriptions are organised, embodies (explanatory) theories. Thus description tends to merge into explanation, and for social anthropological purposes 'we may simply equate the two' (p. 21). On a first analysis, then, even that part of social anthropology known as (descriptive) ethnography contains (explanatory) 'theoretical insight of a general nature'; this strengthens the view that social anthropology qualifies as 'a science only to the extent to which it can explain' (p. 20).

From the observed ethnographic fact the social anthropologist aims to synthesise theories. These

theories are constantly being tested against new facts, often redefined and sometimes abandoned. There are, of course, good theories and bad—theories which account or do not account for the range of facts observed at any time. There are, too, T. H. Huxley's 'beautiful' theories, tragically murdered by an 'ugly little fact'. But the best and most beautiful theory can only account for what is known or observable by existing techniques; and even theories subsequently proved inadequate have added to knowledge or posed problems from which wider knowledge eventually sprang. Thus theories change with observed facts, and observation of facts changes into, and with, theories (p. 22).

One consequence of this attitude to theory is that Nadel attacks the fashionable 'fact worship' among social scientists. By this he means the prevalent empiricist view that what little hope there is for 'scientific' social studies depends on making a determined effort to ground them in a firm basis of brute facts, without any 'premature' attempts to build theories.

Nadel holds that this attitude is fruitless because even "'pure" observation and description' involves 'theoretical insight of a general nature' (p. 11) as we have already seen. This need not imply that

¹ The notions reflect the influence of Weber and Parsons. It can be seen that if the behaviour is 'irrational' in the sense that it is not aim-controlled then it is sociologically 'meaningless', and presumably cannot be socially explained.

Nadel does not himself believe in brute facts. He adds that 'the intrusion of theory into factual observation can be disregarded by all save philosophers' (p. 24); so that the scientist is free to describe and explain these facts. Nadel seems to mean something like this: on the whole the theories which intrude into factual observation are low-level and generally accepted; but nevertheless, if a philosopher were to examine our statements meticulously he would discover (trivial, low-level) theories embedded there.

2.3 *The Methods of Social Anthropology.* Social anthropology aims to describe and explain rational behaviour. The third question was: 'How is this aim to be achieved?' The rest of this part of the paper will be devoted to it.

Explanation, according to Nadel, is a commonsense concept, slightly refined for scientific purposes, but still basically the same (p. 196). 'Anthropology is a science insofar as it explains', he says (p. 191), quoting with approval Carnap's view (*c.* 1934) that a scientific explanation 'consists in deducing (a statement) from the law of the same form as physical laws, i.e., from a general formula for inferring statements of the kind specified.'

Nadel then argues that, when we explain A in terms of D (but not B and C) we postulate an invariant causal relation between A and D; i.e. we formulate a law describing the connection of A with D. Thus an explanation of A consists in describing its connection with D. Nadel generalises this, saying that explanation is 'nothing but complete description' (p. 199).

But closely as these two concepts of explanation and description are related, Nadel insists that there is, nevertheless, a perceptible step from the one to the other. This step, when pinned down more explicitly, seems to consist in the following two things.

First, Nadel follows Mach in holding that it is a characteristic of scientific description that it adds an economical 'plus' to ordinary exhaustive description by "simplifying, schematizing, idealizing . . . the facts"—facts which can "never be perfectly found in reality" (p. 202).¹

And secondly, for any descriptive law to qualify as an explanatory law there must be some 'fitness' or 'requiredness' in the observed regularities described by the law. That is to say, we find it insufficient to be told that two bodies, A and D, always move together, period.

¹ The quote within the quote is from Mach, *Erkenntnis und Irrtum*, Leipzig, 1920, p. 455.

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Because then we always want to ask the causal question: 'Ah yes; but *why* do they move together?' And to answer this we must postulate some such 'fitness' as gravitational attraction. It is perhaps more conventional to call gravity an *explanatory theory*, from which a descriptive law is deduced regarding the effect bodies have on one another. However, little depends on words, and it is clear what Nadel means even if his terminology is unfamiliar.

There are, for Nadel, three levels of what I have re-christened explanatory theories. (i) Theories about the whole of society (his 'social concepts' listed in the next section); (ii) theories about social institutions (functionalism); (iii) theories about individual behaviour for which we must regress to the psychological level. I deal first with (i) and (iii), leaving functionalism a section to itself.

2.4 *Levels of Explanation.* This doctrine of explanatory theories needs to be further expanded. Nadel argues that as every social action is a complex of the psychological, physiological, chemical, and physical processes of individuals it must, ultimately, be explicable in these terms. He describes a hierarchy of the sciences similar to that of Comte (although not credited to him). Nadel holds that mental and physical phenomena, since they constitute the actual level of human life processes, which are necessary conditions of social life, are in some sense ontologically prior to social phenomena. The latter are really 'emergents' from the neural and physiological levels. True, the picture is complicated by the admitted fact that, to some extent, sociological and psychological phenomena interact with each other.

Nadel now applies this theory of emergents to the problem of trying to explain how one social event actually *causes* another. The result is a further theory, his *theory of levels of explanation*; 'one range of problems posed by social enquiry, namely that of finding a mechanical-causal "requiredness" or "fitness" in the social phenomena, can in fact be illuminated by a move to other, lower levels of analysis . . .' (p. 219).

What this amounts to is that Nadel wants to explain how a social action by one individual causes a social reaction by another. To do this he finds it necessary to assume some internal process in the individual whereby an external stimulus evinces a response in the mind of the recipient which is modified and turned into some sort of bodily reaction. He outlines his conception of these internal processes in terms of neural-physiological theories of 'mental energy' and 'action potentials'. This is not the place to discuss them.

That Nadel is not, strictly speaking, a reductionist is evidenced by his belief that there are *levels* of explanation. A reductionist holds that *all* human social behaviour can be reduced to, and is best explained in terms of, 'laws of human nature', i.e. psychology. But Nadel says that institutions and societies cannot be explained in this way. Leaving aside institutions until the next section, we can note Nadel's thesis that as regards whole societies sociologists have developed certain explanatory concepts. He gives as examples of these what might be called 'societal' concepts the following: 'social integration', 'mechanical and organic solidarity', 'social adjustment', 'optimum size and scatter', 'cultural adhesions', 'differentiation in social evolution', and so on (p. 204). Nadel is not, though, particularly clear about whether the entities treated by these concepts are subject to what he calls the 'phenomenal regression' of social studies. By 'phenomenal regression' he means that the subject matter—the phenomena—of social studies 'disappears' when investigation moves to a lower level (p. 212).

Thus there is no necessary contradiction between Nadel's acceptance of holistic societal concepts on the one hand and his argument that social behaviour can be explained in terms of the lower level human processes it emerges from on the other. For his position seems to be something like this. Holistic societal concepts, the whole idea of 'things social', are simply useful explanatory ideas, perhaps useful shorthand. But it must be realised that what we call 'social phenomena' are only *interpreted* as social, for they are really second-order emergents from an interaction of individual neural and physiological processes which is too complex to be specified in full. And these processes are ontologically prior and, perhaps, more ontologically real; and in certain circumstances we interpret complexes of these first-order processes as institutional or societal wholes, to suit our convenience.

2.5 Functional Explanation. Human individuals are not Nadel's 'social atoms' for they are reducible to the various rôles each 'social person' plays in the society. Each social person is a member of various groups (each of which corresponds to a rôle, e.g. married men, hunters), and these groups are 'actuated' through social institutions (marriages, hunts), which are thus defined as 'standardised modes of co-activity' (p. 108). Explaining these standardised modes of co-activity and the interrelations between them is commonly thought to be one of the major tasks of social anthropology. Consequently Nadel's outline

and defence of the 'functional' theory of social institutions is of great interest.

We have already seen that, in Nadel's rather Machian view, science is simply (economical) description of the invariant relations between facts, made intelligible by being subsumed under some 'required' explanatory theory. The explanatory theory under which 'functional' relationships between social institutions are discussed is a much debated matter. Nadel distinguishes at least four distinct 'functional' theories¹ about the relations between social institutions. I will outline the four theories with the help of my own examples.

(I) The first theory is the common-sense one that the function of a social institution is the job that it performs; e.g. 'Among some remote Bedouin the feud still functions as a form of redress in cases of homicide'.

(II) The second theory is a strengthened version of the first to the effect that *every* institution of a culture has a job of work to do in that culture; that no part of the culture is 'random' or accidental or a functionless survival. This theory would tend to take the form of methodological prescriptions; e.g. 'Perhaps it is hard to see what the function of the feud is among the Sicilians now, but there must be *some* reason for its continuance—look closer, try again!'

(III) The third theory is a weakened version of (II) which states that even if every part of a culture has no specifiable job, at least most parts of the culture influence and mould other parts; the parts are interdependent. The degree to which they are interdependent in any particular culture is a matter for investigation. An example of this quasi-mathematical theory in practice would be the statement: 'It has been found that in most of Africa the strength of the institution of the feud is an inverse function of the strength of a centralised judiciary'.

(IV) The fourth theory is an alternative version of (II) to the effect that there is some sort of ulterior (but empirically appropriate) purpose built into all social institutions. This is an interesting example of a metaphysical (or, more precisely, unfalsifiable) theory with, perhaps, valuable methodological consequences; because it urges the anthropologist to seek out such a purpose in all cases, even when, as in the following example, it leads to counter-intuitive results: 'Superficially

¹ He actually conducts his discussion in terms of the meanings of the word 'function' (pp. 368-369). I think my presentation is clearer, and facilitates criticism. (Theory IV is a version of the theory developed by H. M. Gluckman in *Custom and Conflict in Africa*, Oxford, 1955.)

the feud seems to be a mechanism for vengeance in cases of homicide, but the overt hostility of the parties conceals a link binding them together. For if homicide by a member of one group against a member of another provokes only a feud, as opposed to a war, this shows that the groups are less hostile than they would have been if they *had* gone to war. And if a third group should attack either of the two groups in a feuding relationship, the latter will band together to make war on the third group. Thus the function of the feud is to promote a kind of cohesion between groups.'

As these theories are not all compatible with each other some have to be ruled out. Theory (I) can be eliminated¹ as trivial and obvious, theory (II) as obviously false, its useful parts being present in (III) which, in turn, according to Nadel, merges into (IV). (We might add that (III) begs the question of 'fitness' or 'requiredness' altogether, as it postulates a quasi-mathematical correlation, and quite unconnected things can often be correlated.) This leaves him with theory (IV) to which he adds the auxiliary hypothesis that the social institutions of any relatively stable society tend to become more harmoniously integrated. (Further reference to this last assumption will be made in the next section.) An example given by Nadel of this pragmatic theory (IV) is the case of the first fruits ceremony. He points out that as well as all the professed reasons for

permitting the harvesting and consumption of first fruits only after certain sacrifices or rituals have been performed . . . we can substitute an empirical appropriateness for the . . . mystic one. For we should argue that the taboos in question enable men with expert knowledge to control these important activities, so that labour will be efficiently co-ordinated, the premature dissipation of food stocks prevented, and perhaps also the danger to health obviated which might be caused by eating unripe crops (p. 274).

Against the suggestion that the above was just *post hoc* rationalisation, Nadel could have argued that this constituted a genuine explanation because it shows how the social institutions of this culture unwittingly take care of certain matters crucial to the welfare of that culture. He could have shown that when we speak of 'function' in the sense of theory (IV) we are in some way specifying a necessary condition for the survival of the culture. And if the notion that there

¹ Nadel eliminates them on slightly different grounds but, again, besides being stronger, my criticisms make for easier exposition.

are specifiable necessary conditions for the survival of cultures is combined with the minor premiss that this culture has survived then we can legitimately infer that it must have handled these necessary matters somehow. A culture, then, to take the argument one stage further, has the (unintended?) purpose of bringing about the survival of itself. That is, those cultures which survive do so by taking care of certain vital matters better than others. Once we have accepted the simple notion of societies taking part in a Darwinian evolutionary struggle for survival in adverse conditions, we can start speaking in terms of their state of health. Consequently it is possible for Nadel to talk about 'social pathology'.

3 Criticism

3.1 *Background to the Criticisms.* The points made by Nadel which will be criticised are: his conclusion that the aims of social anthropology are: (a) explanation and (b) description, but that the former can be reduced to the latter; his thesis that there are levels of explanation; his interpretation of the laws of social science in an inductivist way, i.e. the view that social laws are factual generalisations which, when they are 'required' or 'fit' constitute explanations; his outline of a functional explanation and its teleological implications.¹

To avoid misunderstanding of these rather negative criticisms, the spirit in which the attacks are launched and the general methodological position from which they are directed should be indicated.

The fieldwork of British social anthropologists is of very high calibre. They go to a strange society and succeed in explaining what often appears to be the most irrational behaviour in terms of the situational logic of that particular social organisation. Their publications, too, are often illuminating and suggestive. Their procedures come into question when they *invert* what Popper calls the *unintended* consequences of some actions; that is, they take the obviously unintended consequence of some action, and proceed to say *per contra* that *the function of that institutionalised action is to bring about those (unintended) consequences* (vide Nadel's discussion of the (IV)th theory of function). Doing this gives a perfectly good notion—events have consequences—a dangerously teleological twist. Using 'function' in this sense implies that the consequences of the event are intended; and that they are

¹ I should perhaps add that without, to my knowledge, distorting his *views*, I have sometimes invented new arguments in support of his views, and have systematically eliminated anything he said which seemed to weaken his case.

intended, and must be intended by someone or something, further implies some kind of supra-individual with motives or purposes or ends which has caused them to come about. This contradicts the metaphysical theory, for which overwhelming arguments have been presented (see the first five items in footnote 3, p. 18) that *human individuals are the only causal factors in society*. From this theory can be derived the axiomatic methodological prescription: 'Explain all social events in terms of human factors.'¹ Metaphysical theories cannot be falsified, of course; but they can be critically discussed as in the rest of this paper.

3.2 *The Subject Matter and Aims of Social Anthropology*. Nadel has taken up the position that the subject matter of social anthropology is rational behaviour. The main aims of social anthropology are to *describe* and *explain* this subject matter. He argues that, in the last analysis, explanation can be reduced to 'economical' or 'fit' description. These aims are to be achieved by the methods of the inductive natural sciences: collecting facts, using these to eliminate dubious hunches, then synthesising them into true natural laws.

Against this I shall maintain that the subject matter of social anthropology is problems; that its aim is to solve these problems; and that this aim is best pursued by the rational or critical method of advancing bold hypothetical solutions and doing our best to criticise them.

Earlier it was suggested that social anthropology arose from, and still centres around, the kind of *problems* mentioned in 2.1. Few would dispute this until, perhaps, they see how it implies that the aim of the subject is solving these problems; that is, explanation. Description, it seems, does not come into it. So the important question now is this.

Has Nadel made much of a case for accepting description as an aim, i.e. as an end in itself? Is it more of an end in itself than the formulation of problems as clearly and simply as possible is an end in itself (important though it is)? Is it not rather a means to a further end, namely the attempt to solve the problems? Nadel's elevation of description to an aim, then, now depends entirely on his attempt to reduce explanation to description; and some reservations are in order with regard to this. It is easy to see what he means, of course. When we don't understand something we want to know its cause, and an ex-

¹ The end of the paragraph is based on J. W. N. Watkins's note 'The Two Theses of Methodological Individualism', this *Journal*, 1959, 9, 319-320

planation of *why* p causes q will in fact be a description of *how* p causes q . The explanation will be a deduction from a set of statements including one describing a law of nature, whether that be an eternal verity laid up in heaven, or simply a fortuitous regularity in the face of cosmological indeterminism. But Nadel himself is a little worried by the success of his reduction so he adds that scientific description is both more economical than ordinary description, and that it must have an element of 'fitness' or 'requiredness' if it is to be satisfactory. His own subtlety has caused him to miss the main point here. This is the evident fact that when we have the causal mechanics behind some event described to us, an element of *understanding* is imported; our knowledge of the causal antecedents of the event allows us, provided the 'why' questions are not carried back too far, to say, to all intents and purposes, *why* p has caused q .

In other words this assimilation of the notion of explanation to description, although in some ways correct, is misleading. Subsuming explanation under the descriptive statements used *in such* an explanation tends to blur the fact that science has an explanatory, as well as a descriptive, function and that these two things are not the same. In this respect Nadel's whole treatment of explanation and description, like many attempts to refine or explicate our ordinary-language concepts, seems rather fruitless. If the common-sense view of explanation is not distinct from description when analysed closely, and given that a distinction seems necessary, all the more reason for sticking strictly to the technical notion of explanation invented by logicians to describe what happens in science. Nadel makes surprisingly little use of Carnap's model of explanation, which presumably was introduced into methodological discussion just because ordinary language was found to be too imprecise. Explanation in this technical sense means the process of deducing a statement describing what is to be explained from a universal statement (together with certain statements of initial conditions). Now this process of *deduction* cannot be reduced to description, even though all the statements in the deduction are descriptive ones. Also, that this technical notion in fact approximates closely to our intuitive idea of what it is to explain something should become clear in the next section.

There is only one other point to be made in this connection. Were description an end in itself it would have to be exact and 'objective' or 'scientific'. But such description is a scientistic myth; or, in other words, all description is selective and our selection is governed by

our interests; that is, the problems we are interested in solving, the theories we are interested in testing. So scientific description *cannot* be the aim of social anthropology.

Thus the only aim left for social anthropology is explanation. Nadel's view that social anthropology is a science has been accepted; together with the view of those philosophers of science who hold that the generalised aim of all scientific activity is to find testable, causal explanations of the kind outlined above. Description plays two main rôles in this. The first is in the formulation, as clearly and simply as possible, of problems. The second is in the marshalling of facts with which (or against which) to test the hypothetical solutions to these problems.

3.3 *The Methods of Social Anthropology: Explanation.* In order to examine Nadel on explanation in detail his own example of the joking relationship, a typical anthropological problem, will be analysed.

In many primitive societies grandfather and grandchild stand in an especially intimate relationship, which is friendly and of equal footing, and implies that familiarity and privileged disrespect which anthropologists call 'joking relationship'. Between grandfather and grandchild there is none of the disciplinarian attitude and demand for respect which characterises the relationship between a father and his boy and their respective generations. On grounds of general knowledge we suspect where the relevant conditions for this state of affairs may lie. They would seem to lie in the fact that the grandfather stands, by his age, on the borderline of social usefulness and, by his generation, on the border of the effective family group. He does not *need* to exact the respect of the growing generation, while the father, who directs the family and the education of the young, *must* do so. The grandfather-grandchild relationship would therefore seem to offer a *relief* from the sterner atmosphere of authority which otherwise dominates the relationship between the child and the adults of his family (p. 235, *italics mine*).

The problem is the existence of a 'joking relationship', rather than a relationship of respect, between grandfathers and grandchildren in many primitive societies.

The explanation—'on grounds of general knowledge'—is this. Because the grandfather is not very useful socially and because his age puts him on the outer fringe of the family group, he does not need to exact the respect of the younger generation. In other words, because his responsibilities are low the grandfather can afford to offer a 'relief' from the sterner atmosphere which otherwise pervades in adult-child relationships.

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Such a hypothesis is well suited to comparative testing, but before such empirical tests are resorted to it should be critically examined to see whether it is a *satisfactory explanation*.

The explanation has the following form:

- (i) LAW STATEMENT: Adults exact from children the amount of respect they need to maintain their position.
 - (iia) STATEMENTS OF
 - (iib) INITIAL CONDITIONS
 - (iii) CONCLUSION: Therefore grandfathers do not exact respect from grandchildren.¹
- | | |
|---|--|
| } | Grandfathers do not need respect from children |
| } | Grandfathers are adults (analytic). |

This should make the structure of Nadel's explanation clear.² True, the law statement should specify more precisely the limiting conditions of family organisation, political structure, and so on. Nevertheless we see at once that the law statement is problematic. Leaving aside the question of whether respect is not *desired* as well as needed, we can see a mistake in the notion that the respect a child pays to an adult depends solely on the wishes of the adult. After all, respect is given as well as received. Children are not automatons who respond immediately to the wishes of an adult—a child very often does not do what his elders want him to do—both actors, in fact, contribute to the pattern of who respects whom. This argues that the law statement extracted from Nadel's text oversimplifies the story of why certain socially normal patterns of respect exist; and because it ignores some obvious factors intrinsic in respect-relationships, is not a very satisfactory explanation of the problem. Nadel rather slurs over the question of why people conform to this norm; that is, what factors maintain it. The whole matter is difficult, certainly; all the discussion tries to show is that *purely methodological criticism* indicates further thought is needed before empirical testing will be worthwhile. For example, it is not even clear whether an explanation of joking relationships is best sought in terms of the unintended consequences of certain other

¹ Where (i), (iia) and (iib) constitute the *explicans* and (iii) the *explicandum*.

² It has been pointed out to me that Nadel's formulation of this argument is not as good as it could have been, and that he was writing in abbreviated form for an audience with specialist knowledge, some of whom have produced better versions. This means that my criticisms of his example cannot be as strong as I should like because although Nadel uses very few examples they were obviously chosen with great care and are not therefore to be lightly modified or 'improved'.

factors in the situation of the two actors; or in terms of a conscious intention of the actors themselves to bring about the state of affairs which is thought in need of explanation. (Such a conscious intention is *almost* implied by Nadel when he speaks of 'relief' from a posited 'tension' in the authority-relationship normally obtaining between children and adults.)

3.4 *Levels of Explanation.* In section 2.4, in order to defend the theory of levels of explanation, Nadel's argument concerning the ontological status of social reality was given. To avoid pursuing the topic in that direction a logically weaker version of the theory will be dealt with to the effect that there are some sociological problems which can (and some which must) be explained on other levels, i.e. the levels of psychology and physiology.

It has already been argued that Nadel is not strictly speaking a reductionist, yet his doctrine of a phenomenal regression seems to come to much the same thing. He argues that some social phenomena 'disappear' when they are described in psychological and physiological language. This 'disappearance' means that the translation from sociological to psychological and physiological language can be carried out *without loss of informative content*. But this kind of translation would be an explanation in itself. Nadel accepts this point when he argues that the model we employ of a 'rational action' itself entails the assumption (i.e. must ultimately be explained in terms) of psychological mechanisms like 'mental energy' and 'action potentials'. Perhaps it does, but does not this beg the question of whether such models of rational actions are sufficient to explain social behaviour? That is, perhaps we can regress to other levels of explanation or description, but if we do, are we answering the same questions, are we tackling the same problems? Clearly not, for when we ask why there is a joking relationship in society *x* we are not likely to be satisfied with an answer like: 'Because people in *x* have a psychological disposition to do so'. Because this is the point. What we want to know is what it is in *x* which causes the people in *x* to have this psychological disposition, since it does not appear to be a universal human attribute. And since any answer will involve both heredity and environment, and the latter is partly social, it can be seen at once that psychology cannot be used to explain 'things social', for it is in its turn dependent upon them.

And if it is remembered that the problems of the social sciences largely concern the unintended consequences of our actions, it will

be clear that psychology is irrelevant. For how can the *repercussions* of our actions, and the unintended repercussions at that, be reduced to psychology or explained on any other 'levels'.

There remains a decisive and possibly unanswerable objection to the whole theory of levels of explanation. The argument rests on the fact that (as Nadel himself points out, although he does not see the implications) there is some interaction between the different levels. That is to say there is interaction between sociological and psychological (and perhaps physiological) phenomena. In other words, granted that social life is to some extent a product of things happening on the psychological and even physiological levels; it must not be forgotten that in turn there is a reverse process whereby things happening on the social level influence psychological (and even physiological) processes. Nadel fails to notice that this interaction contradicts the theory of levels: it contradicts the notion that any one level has priority over any other,¹ and with that the idea that any one level can explain or be reduced to any other. For if psychology and physiology are necessary conditions for sociology, and there is interaction, then sociology becomes a necessary condition for psychology (and perhaps in more complicated cases, physiology; e.g. it is possible to envisage circumstances in which anti-social tendencies might result in a person dying). The levels are thus mutually interdependent and therefore co-present. No event can be satisfactorily explained on any other level than its own. Any attempt to explain, e.g. the social environment in terms of psychology will have to include the social environment which contributed to the psychology, and thus the explanation will be circular. And if psychology is taken to mean neural processes (as Nadel at times seems to) then the social environment can influence neural processes, i.e. physiology, and the circularity of the theory of levels is further reinforced.

The upshot of this analysis is that there are no levels; there are only problems and explanations; and explanations are best judged solely by the criterion of whether or not they provide satisfactory answers to the questions asked. The notion of moves to lower levels of explanation is irrelevant here, for what we are interested in is causal explanation. An analysis of the individual's situation is sufficient to explain social behaviour causally. It looks as though what Nadel is really gnawing away at, with his discussion of levels, is the totally different question of mind-body interaction. There are good

¹ Including even the ontological priority not discussed.

reasons for supposing that a solution to this problem is in a certain sense logically impossible.¹ Be that as it may, Nadel has no warrant for the way he blandly assumes monism and then tries to build it into social anthropology. Had he realised what he was doing² his argument would doubtless have been different.

Nadel is in many ways a good example of his own criticism in that there is a clash ('lack of agreement') between what he advocates and what he does; e.g. in handling the joking relationship, as we saw in section 3.3, he uses neither holistic concepts nor psychology, and his explanation does not seem to be functional (this is discussed in 3.6). Now his psychologism has been rejected as circular. If we are also to reject his holism for individualism we must answer the question: is there a methodological individualism which is not psychologistic? This crucial methodological ground has been well trodden by Hayek, Popper, Watkins, Gellner, and now Agassi.³ It is, however, a possibility which Nadel overlooked, although he came near to it in his joking relationship example. How *would* it work out in terms of the joking relationship?

Presumably a psychological individualist explains this by means of some such notion as 'laws of human nature' concerning affection and respect, and how these emotions are encouraged in certain directions in some societies and in other directions in others—the joking relationship society being one of the others.

This explanation is already circular, of course, because sociological terms have had to be reintroduced. The trouble is caused by the fact

¹ I must thank Mr Watkins for making this point clear to me.

² That he did not is surprising in view of the extent of his knowledge of the philosophical literature revealed in the bibliography.

The mind-body problem arises in Nadel's book only because he neglects his own assumption that we are considering rational behaviour (we might, following Popper, call the assumption 'the rationality principle'; see section 2.2, text to footnote 1). Given this assumption we do not need to know anything about the mental processes involved in taking a decision. All we need, to assess its rationality, is the information in the light of which it was taken, given the ends to be attained.

³ F. A. von Hayek, *The Counter-Revolution of Science*, Glencoe, 1952; K. R. Popper, *The Open Society and Its Enemies* (3rd edn.), London, 1957, ch. 14, and *The Poverty of Historicism*, London, 1957, sections 29, 31, 32; J. W. N. Watkins, 'Ideal Types and Historical Explanation', this *Journal*, 1952, 3, 22-43, and 'Historical Explanation in the Social Sciences', this *Journal*, 1957, 8, 104-117; E. A. Gellner, 'Explanation in History', *Artist. Soc. Supp.* 1956, 30, 'Dreams and Self Knowledge', 157-176. I want to thank Dr Agassi for letting me read his paper 'Methodological Individualism' (*Brit. J. of Sociology*, 1960, 11, 244-70) in MS. I found it very helpful.

that psychologism cannot account for social institutions (see the authors cited).

A methodological individualist would ask (among other things) what factors in the situation of an anonymous grandfather in this society make him want (i.e. act to bring about) a different relationship with his grandchildren than with his children. The answer will show how the joking relationship is tied in with other features of the society which help maintain it by creating circumstances which make the grandfather decide to act in accord with, rather than against, the traditional norm. (This explanation assumes only the rationality principle assumed by Nadel himself—see footnote 2, p. 18—and that we can know the grandfather's aims and the state of his information.)

3.5 *Induction and Social Laws.* Induction, by which is meant the generalising of laws from collections of particular facts, is an interpretation of the methods of science not accepted here. But it must be discussed because Nadel contradicts himself over it in a rather important way. He maintains induction on the one hand by claiming that theories are somehow synthesised from facts; then on the other hand (p. 224) he in effect gives up induction by introducing a notion of 'pre-conceived ideas', which are similar to Bacon's *anticipatio* ('anticipations' is Nadel's term). These 'anticipations' are 'some preliminary hypothesis or suspicion as to the kind of correlations likely to prove relevant'. But if he has a hypothesis, however preliminary, the deductive consequences of which will presumably be tested against the facts, then he is using the hypothetico-deductive method which is logically incompatible with induction; and so he must abandon the latter.

Underlying the contradiction between the inductivist desire for deductivist certainty, and the necessity of having uncertain preliminary hypotheses, is a serious problem. It is, in fact, Bacon's problem of the tendency all theories have to verify themselves. If we hold false theories our minds will be prejudiced by them and any evidence we collect to test these theories will be seen in their light, and distorted in their favour, like looking through coloured spectacles. The theories will only allow us to see favourable (or confirming) and neutral evidence, rendering us blind to the rest; and this distortion will become so acute as we build up more and more theory-impregnated facts that we will never be able to get rid of the false theories.

According to Bacon this problem can only be solved by ridding the mind of all theoretical 'prejudice' so that it is pure the

observation of facts. When the pure mind observes, theories will somehow leap into it. Any theories which come into the pure mind will be *ipso facto* true because they will be derived from unprejudiced observation of Nature, and Nature does not lie.

Nadel's difficulty with this arises, perhaps, because he can see the fruitlessness of the empiric's collection of facts, and yet he is not convinced by the 'pure mind' solution. So he reintroduces the (tabooed Baconian) idea of 'anticipations'; suspicions or hunches about which facts will be significant. But then these anticipations at once lead him into precisely the verificationist trap that Bacon strove so hard to avoid. So the problem Nadel now has to solve is how to prevent hunches about which facts will be significant from becoming prejudicial; without, that is, going so far as Bacon and systematically eliminating them.

Nadel's solution to the problem is to minimise it. He thinks that we clear our minds of theoretical prejudice by an effort, and that the theories left after this has been made will not be prejudicial. And if this is the case then only a philosopher need attend to such minimal colouring as the mind still suffers from. He seems to think that once we have made a *determined effort* to exorcise the prejudicial effect of theories, and tried to get at really 'pure' nuggets of hard fact, then we can consign the rest of the task to the philosopher—who deals with details. But just by thinking that the prejudicial effect of theories can to all intents and purposes be eliminated by an effort, he commits the cardinal sin. For Popper has strengthened Bacon's argument to make the point that we can *never* escape from theories; our minds are, perhaps, naturally, inescapably prejudiced. The best we can do is face this situation squarely and do our utmost to make explicit any theories we hold so that they can be criticised. We should never make the mistake of believing that we have reached rock bottom where 'the intrusion of theory into factual observation can be disregarded by all, save philosophers' (p. 24).

There are several other points concerning Nadel and induction. It is fairly well known that induction can be shown to be both logically impossible and to lead to contradictions. Various philosophers have tried to escape from these criticisms but none have succeeded. Yet if Nadel is going to accept Bacon's solution to the problem he should take care to avoid the very pitfalls Bacon himself pointed out. In fact he rejects Bacon's method of avoiding the verificationist trap and substitutes a solution of his own which plunges him straight back into it.

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Another question still remains. Whether or not in the light of the above criticisms Nadel would have been able to retain his view that social laws simply come down to 'fit' or 'required' correlations? The view taken here is that in science 'law' is most usefully confined to physically necessary causal connections between phenomena. Nadel might have countered this by asserting that social laws are different from the laws of natural science precisely in that they describe contingent regularities and not necessities; in which case he would be wrong. Wrong simply because there exist counter-examples: e.g. 'All social changes create vested interests which resist further change'; and many principles of economics. And he could hardly have argued that in social science both contingent and necessary regularities are called laws because the difference between them is not important. I hope this will not be taken as merely a dispute about using the word 'law'. It is actually about whether the aim of science is achieved by means of inductive generalisations, or by strict laws of physical necessity; and whether the two are usefully kept distinct. My position is that the two *must* be kept distinct and that science aims at the latter. This paper is not the place to justify this position but perhaps enough has been said to indicate that Nadel's view is unsatisfactory.

It would seem, then, that Professor Nadel thinks the science of social anthropology proceeds by means of inductive generalisation, a view rejected in this paper. He could have countered this by asking how social anthropology does proceed if it does not use induction.

Let me briefly indicate an alternative view of the logic of the social anthropologists' procedure. Like Nadel I think that social anthropology is a science, and I hold that the aim or task of science is to explain the world. Social anthropologists seem to me to be largely concerned with two categories of problems which I shall call *local* and *general*. Locally they are concerned to explain problematic situations in any particular society. They might, for example, find a society in which all senior males are paid respect, with the sole exception that between grandfathers and grandchildren there is a joking relationship. This is a local problem of explaining conformity to the established norms of behaviour in a given society, in terms of the aims and situation of anonymous individuals. On the general level, however, their problems arise from comparing societies and trying to understand why they differ. Why, e.g. one tribe has a joking relationship between certain relatives, but the neighbouring

tribe, although in other respects similar, has not. Or, for example, how law and order is maintained in certain Polynesian societies despite the absence of government machinery similar to that in the West. The former requires a historical explanation; the latter is a problem of the unintended consequences of institutions.

3.6 Functional Explanation. As to functionalism. Nadel claims to be most interested in the (IV)th functional theory, namely the one which looks for some ulterior purpose in social institutions. Yet he ignores this theory when he tries to explain the institution of the joking relationship. Perhaps he realised how little real explanatory power there is in the (IV)th theory of function. I suggest, in fact, that he is really interested in theories (II) and (III), i.e. the notions that *all* social institutions have a rôle in society, and that there are discoverable invariant relations between social happenings. The first theory is metaphysical but methodologically useful, the second theory has been fully overhauled in the previous section. In the joking relationship example Nadel seems to neglect functional explanation for the logic of the situation which he does not handle too well. Although, in all fairness, we *could* argue that the grandfather's lack of function, the fact that he has few rights and duties towards his grandchildren, and expects few from them, allows him to have a more friendly, less disciplinarian relationship with them. Yet this explanation cannot be a part of Nadel's (IV)th theory of function, and it presents the following difficulties for theories (II) and (III). What is the joking relationship's sociological rôle? What is the joking relationship correlated with?

The joking relationship example has shown enough and need not be gone into further. We can now see that there is at least one problem which functionalism cannot satisfactorily explain. That is all we need. Functionalism is a very important doctrine and in some forms has *solved* extremely difficult problems. But it does not, by any means, solve all the typical problems of social anthropology,¹ and if it is Nadelian functionalism (theory (IV)) it seems not to solve any problems at all.

3.7 Purposive Explanation and Teleology. By and large Nadel does not exaggerate the explanatory power of holism; if by 'holism' we mean speaking metaphorically about institutions as though they were wholes, when describing the situational logic. But severe reservations

¹ Cf. Kingsley Davis for the opposite view in 'The Myth of Functional Analysis as a Special Method in Sociology and Anthropology', *American Sociological Review*, 1959, 24, 757-772

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about its use in other contexts have to be lodged; these are well known and need not be rehearsed here.¹ Instead, I wish to give an illustration of the way holism (of the kind at the back of the (IV)th theory of function), quickly leads to uncritical explanations.

Society and culture are made and worked by man. May we not assume that they are made and worked for man? The Great Engineer is merely Man in the abstract, and the Intelligence at the back of all things social, the Human Mind writ large (p. 368).

As R. Needham has pointed out² this disturbingly Whiteheadian passage would be less troubling if we knew what the capitalised entities were. What do these metaphysical conjuncts about the Great Engineer and His Intelligence explain? How do they differ from the belief that the universe reveals a Divine Pattern? It is surprising that people should believe such hypotheses explain anything; the fact is that they explain everything, and that is too much. Any hypothesis which explains everything, gives no causal, *testable* explanations at all.

We come now to the final objection to Nadel's (IV)th theory of function which, by explaining an institution in terms of what purpose it serves, is teleological. And teleological explanations are not the causal explanations in which science is interested. For they are ultimate and final explanations and Popper has argued convincingly that there is no place for ultimate explanations in science;³ there is always room for better and higher-level hypotheses. Emmet's contention that if functionalism is teleological then it is 'teleological in complicated ways' does not seem to escape the criticism.⁴ And weakening the theory to make it less open to criticism also makes it trivial.

4. Conclusion

The principal object of this paper has been to outline and criticise Professor Nadel's view of the aims and methods of social anthropology. The interest shown by Nadel and other leading British social anthropologists in such basic problems strikes me as a result of the development of the subject into a major social science.⁵ Nadel has thrown light on many difficult and interesting problems. His principal thesis

¹ See K. R. Popper, *The Poverty of Historicism*, London, 1957, sections 23 and 24.

² R. Needham, in a review of Nadel in *Man*, 1951, 51, 130-132.

³ K. R. Popper, 'The Aim of Science', *Ratio*, 1957, 1, 24-35.

⁴ Dorothy Emmet, *Function, Purpose and Powers*, London, 1958, p. 7.

⁵ Methodological preoccupations are either a symptom or a disease—I prefer to regard them as a symptom.

that social anthropology is a science has been accepted, so far as it accords with Popper's view of science; where there is a clash Nadel has been criticised. The point of all this being that social anthropology still has great untapped potential; and, as Nadel argues, it is untapped at least in part because of faulty methodology. My personal conviction that the way out of the impasse for social anthropology, as for the other social sciences, lies by way of methodological individualism has been strengthened by working over Nadel's arguments.

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STATISTICS AND SELECTION *

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I

THE shadow of Archbishop Paley and the argument from design still broods over evolutionary theory. Although the inference is now not to a Contriver but to Natural Selection, the premise from which the argument starts is still the same: the phenomenon of adaptation, the remarkable fashion in which tissues, organs, organ systems of living things appear 'suited to' the functions they perform. This premise is sometimes baldly stated in the identification of animals with machines, sometimes left implicit, as with Darwin, in the direction and emphasis of evolutionary theory, but its rôle is crucial in either case.

What I want to talk about here, however, is not the axiom of adaptivity, as one may call it, in its whole range, but one particular form of evolutionary theory in which the pervasive importance of adaptation has the status neither of an explicit postulate nor of an implicit belief, but of a proven theorem. This is, or seems to be, the case in Sir Ronald Fisher's *Genetical Theory of Natural Selection*,¹ a work which has played a decisive part in the rise of neo-Darwinism or, as it is sometimes called, 'the synthetic theory'. It is the conceptual structure of Fisher's theory that I want to examine. Again, not all theories of evolution, not even all neo-Darwinian theories share this structure; but Fisher's argument has been so very influential that it seems worthwhile to examine it in some detail.² I shall take the mathematical core of the theory for granted, but want to examine the concepts carried by the mathematical formalism.

* Read to the British Society for the Philosophy of Science, May 16th, 1960

¹ Oxford, 1930 (New York, Dover 1959)

² It is often said that Fisher has 'proved mathematically' the truth of neo-Darwinism. The confidence with which the 'synthetic' theory has been asserted on the basis of Fisher's argument is reflected, for example, in the contributions by Huxley, Fisher and Mayr in Huxley, Hardy, Ford, *Evolution as a Process*, London, 1954; or in the conclusion of the third edition of de Beer's *Embryos and Ancestors*, Oxford, 1958, or in P. M. Sheppard's *Natural Selection and Heredity*, London, 1959—to mention but a few examples among many.

As Fisher writes, 'the rigour of the demonstration requires that the terms employed should be used strictly as defined'.¹ Yet his argument, as a demonstration of the effectiveness of natural selection throughout the evolutionary process, depends, as I hope to show, on the use of his fundamental concepts, not always 'strictly as defined', but in a number of senses, one 'strict' and several more comprehensive.

2

It is the principal thesis of all Darwinian and neo-Darwinian theories that evolution is the result of the joint action of random variation (or mutation) and environmental pressure (or natural selection). How is this basic thesis supported by Fisher's argument in the *Genetical Theory*? I am not asking whether evolution is in fact a function of random variation times natural selection, but whether or to what extent Fisher has demonstrated that it is so. The crucial text we have to consider is chapter two, on the Fundamental Theorem, although much that is said in the chapters on the evolution of dominance and on mutation and selection is also relevant.

What is to be demonstrated in chapter two, Fisher tells us at the start, is that *the rôle of improvement of any species of organisms in relation to its environment is determined by its present condition*. 'Improvement in relation to environment' seems to mean 'increased adaptation'; but the term 'adaptation' is formally introduced only later, where it is defined in terms which appear to presuppose the concept of 'improvement':

An organism is regarded as adapted to a particular situation, or to the totality of situations which constitute its environment, only in so far as we can imagine an assemblage of slightly different situations, or environments, to which the animal would on the whole be less well adapted; and equally only in so far as we can imagine an assemblage of slightly different organic forms, which would be less well adapted to that environment.²

In other words, an organism is regarded as adapted, if one can imagine its condition as an improvement over another possible condition that would be slightly less favourable.

¹ Sir Ronald Fisher, *The Genetical Theory of Natural Selection*, New York, 1959, p. 38. I am following the Dover edition, which is in part revised, but not, with one exception, to be noted later, in ways that are philosophically relevant.

² Op. cit., p. 41

Thus the concept of improvement is central to the argument, and we must find out what it means. And there is also the question, in what sense the present condition of an organism 'determines' its 'improvement in relation to its environment'. Does 'determine' here mean 'cause'; if so, how, or if not, what does it mean?

3

Fisher's procedure is to establish a table of reproduction, analogous to a life table, and to specify a parameter of population increase m , expressing the 'relative rate of increase or decrease of a population when in the steady state appropriate to any such system',¹ that is, when the distribution of age groups in the population is constant. Fisher calls this the 'Malthusian parameter of population increase', and it is said to 'measure *fitness* by the objective fact of representation in future generations'.² Now suppose we are measuring a particular character, such as tallness, in a population. And suppose we assume, as in the light of orthodox genetics we do, that all persistent changes in organisms are produced by changes in their genetic make-up.³ Then we may calculate the average effect on the character in question of a given gene substitution, and also the effect of this one gene substitution on m (that is, on the chance of leaving posterity). We then calculate a summation of these gene changes *and* their effects on m for all the genes in the population, and we get what Fisher calls the 'genetic variance'—in this case for example, the genetic variance of tallness. Now suppose further that what we are measuring is not tallness but fitness itself. We then find, by further calculation, that : '*The rate of increase in fitness of any organism at any time is equal to its genetic variance of fitness at that time*'.⁴ This statement Fisher describes as 'the fundamental theorem of Natural Selection'.

' m ', or the 'rate of progress of a species in fitness to survive', is, as Fisher says, 'a well-defined statistical attribute of a population'.⁵ It is the increase (or decrease) in the chance this population has of leaving posterity at some future date. 'Progress' here means simply this statistical increase or decrease, and 'fitness' in this context would seem

¹ Op. cit., p. 26

² Op. cit., p. 37

³ I am speaking here (as Fisher is doing) of genic inheritance. The question of cytoplasmic inheritance introduces another dimension altogether into the evolutionary problem.

⁴ Loc. cit. (my italics)

⁵ Op. cit., p. 40

to be the same for all organisms: it is a statistical measurement, which has, so far, nothing to do with the particular structures or functions of particular organisms as suited to particular environments. In fact, it has, so far, nothing to do with environment at all, nor with 'improvement' in any sense other than the very restricted quantitative one: that a gene or an individual or a population may be said to be 'improved' when its chance of having descendants at some future date has risen. This is 'improvement' in the sense in which a patient has improved when he is more likely to survive.

4

If this were the whole story, there would be no problem. It would be possible, as indeed happened far and wide in the wake of the publication of Fisher's book, to make valuable calculations, resembling actuarial tables, by which to record and, up to a point, to predict the gradual increase or decrease of certain genetic factors in Mendelian interbreeding populations. Some of the great array of experimental work in which Fisher's techniques were in fact applied is referred to in this edition.

It seems odd, however, to speak in this connection of 'improvement in relation to environment' or of 'Natural Selection'. 'Improvement in relation to environment' suggests a correlation which has so far not been considered; and similarly, 'Natural Selection' seems to describe not a purely numerical relationship, but something qualitative as well; that is, the elimination of characters less well adapted to a particular environment in favour of those slightly better adapted to that environment.

In what sense, then, is the Fundamental Theorem a theorem of 'Natural Selection' (concerned, *a fortiori*, with 'improvement in relation to environment')? In introducing the notion of 'reproductive value', that is, the probability that a given individual or a given age will produce descendants in respect to some future date, Fisher remarks that this value is of interest, since '*the action of Natural Selection must be proportional to it*'.¹ This is indeed true. If, as Fisher points out, the reproductive value of the inhabitants of Great Britain decreased between 1911 and 1921 as a result of the First World War, then whatever selective effects *might* take place, *must* take place only upon that declining

¹ Op. cit., p. 27

group; and therefore the action of Natural Selection would be proportional to the decline. And conversely, where there is Natural Selection taking place, its operation can be investigated and measured by the use of Fisher's techniques. The fact of industrial melanism, for example, has been known for many years; but Kettlewell has recently studied it with great care and precision in terms of modern population genetics.¹ Thus if normal peppered moths (*Biston petularia*) are taken by predators in industrial areas in greater numbers than the darker *carbonaria* mutants, then the increase of *carbonaria* and the decrease of light coloured individuals can be recorded as a decrease in reproductive value of the normal form (which is more likely to be eaten before reproducing—and its offspring also and so on) or an increase in the reproductive value of the *carbonaria*, that is, an increase in the probability that there will be *carbonaria* in future. In other words, once we know that selection has *in fact* taken place in a particular situation, we can record and elaborate this knowledge in the statistical terms of Fisher's theory, and within the limits of selection experiments we can apply Fisher's technique to predict their results.

This statement about reproductive value, moreover, holds equally for the fundamental theorem itself, which generalises the notion of increase in reproductive value for an active interbreeding population, and equates it with the increase in probability of survival of the particular genes forming the genetic constitution of the population in question. Thus *either* (as in the decline of the British population) the fundamental theorem expresses the limits within which Natural Selection can operate; *or* (as in the case of industrial melanism) the occurrence of Natural Selection (as known from other sources) entails the truth of the fundamental theorem.

5

But it is not at all clear that the fundamental theorem entails Natural Selection. In fact, in the example of reproductive value cited by Fisher, the effect of war, the change in reproductive value is notoriously one in which what would appear to common sense to be the better adapted are eliminated in favour of the less well-adapted: the very opposite of a natural selection effect. For natural selection, according to Fisher's own insistence here and elsewhere, expresses the tendency to

¹ H. B. D. Kettlewell, *Nature*, 1955, **175**, 943; *Heredity*, 1955, **9**, 323, and 1956, **10**, 287; *Proc. Roy. Soc. B*, 1956, **145**, 297

increased, not to decreased adaptation.¹ More generally, we should recall also that, as Darwin himself admitted, natural selection is applicable only to such characters or functions of living things as are both heritable and adaptive (whether for good or ill)—that is, relevant to viability or the reverse in a given environment. But we might record the increase or decrease of all sorts of characters in populations without regard to their 'adaptive' value one way or another. Suppose, for example, that red hair is increasing, and hence the probability of larger numbers of future redheads is also increasing; this does not in itself tell us anything about the usefulness of red hair, unless we know from some other source that all characters that are increasing in the population must be of use to it. Darwin was well aware of this restriction on his theory, but was confident that on the whole most characters of organic beings are adaptive. And what Darwin demonstrated in the first four chapters of the *Origin* was that, given the largely adaptive nature of organisms, and variation, and inheritance, and Malthusian population increase, natural selection necessarily follows.² But Fisher's statistical proof does not appear to be rich enough in its premises to permit this conclusion. In terms of the 'strictly defined' concepts it uses, the fundamental theorem is not a theorem of natural selection, but a statistical device for recording and predicting population changes. Nor is the situation altered by calling such changes 'genetical selection'. We must still distinguish between 'genetical selection', which is purely statistical, and Darwinian selection, which is environment-based and causal. They remain two distinct concepts with a common name.

6

It is important to keep this distinction in mind when interpreting the use made by Fisher himself and by others—notably, for example, Huxley, Haldane, Simpson—in applying his formula. The fundamental theorem has been stated, Fisher says, for 'idealised populations' in which 'fortuitous fluctuations in genetic composition have been excluded'.³ He then calculates the error due to such 'fortuitous

¹ Or if, alternatively, we say that in war it is the ('normally') mal-adapted who are better adapted, and vice versa, then we are using 'better adapted' to mean 'surviving' and are saying only: 'those who survive, survive'.

² The demonstrative character of Darwin's argument was pointed out by C. F. A. Pantin (in *History of Science*, London, 1953; cf. the discussion by A. G. N. Flew, in *New Biology*, 1959, no. 28, pp. 25 ff.

³ Fisher, *op. cit.*, p. 38

fluctuations'. The standard error turns out to be $\frac{1}{T} \sqrt{\frac{W}{2n}}$, where

T is the time of a generation, n is the number of the population, and W the rate of increase in fitness of a population measured in terms of the summation of the effect of the increase in particular genes on m . In terms of this formula, it turns out that the rate of increase in fitness becomes irregular only when it is so slight as to be of the value of $1/n$. Only then, in other words, do the random fluctuations produced in a single generation by mutation (given random mating) affect the exactness of the statistical measurement, or, as Fisher puts it, 'the regularity of the rate of progress'. And more than this: if a long enough time is allowed, even this irregularity will recede to vanishing point. For even if the value of m for different genotypes were so delicately balanced in a given generation as to show only a $1/n$ distinction, and hence a fluctuation in m and the summation of the effects of genes on m , still over a span of 10,000 generations, the deviations from regularity would be just a hundredfold less.

Now this argument is taken to show that 'very low rates of selective intensity' are effective in nature. But if we have interpreted the fundamental theorem correctly, what it shows in fact is that very slight trends in the frequency of characters in populations may be recorded if they persist over a long period of time. Whether, in nature, such trends are the result of a process that can reasonably be called 'natural selection' is another question altogether.¹

How does Fisher deal with this question? What he argued in his 1932 paper in *Science Progress*² was that such trends are not, in large populations, the result of mutation alone (or even chiefly), since mutations are too infrequent and advantageous mutations still more infrequent, and therefore it must be natural selection that directs the trends which his statistics describe. This follows, however, only if mutation and natural selection are the only two possible causes of evolutionary change: and that, Fisher is arguing, is what the theory is supposed to prove, not to presuppose.

Nor is the argument of our present text (i.e. the *Genetical Theory*) more satisfactory, for it moves in an even narrower circle.³ Since

¹ If one has set up a selection experiment, of course they are so.

² R. Fisher, 'The Bearing of Genetics on Theories of Evolution', *Science Progress*, 1932, 27, 273-287

³ *Ibid.*, pp. 131-132

only mutation rates around the $1/n$ value are 'selectively neutral', Fisher says, random variation, i.e. mutation, will very seldom indeed be an effective factor in evolution. But what does 'selectively neutral' mean? It means precisely: with a mutation rate, that is, a rate of gene substitution, around the $1/n$ value or less. Similarly, '1 per cent selective advantage' means 1 per cent increase in numbers of a particular gene substitution. Any mutation rate more substantial than $1/n$ therefore automatically becomes 'selective advantage' and so the product of selection, not mutation; and any lesser mutation rate remains 'selectively neutral', that is, counts as mutation, not selection. All this will be a more than verbal argument only if we know from other evidence that selection, in the environment-related, Darwinian sense, is always the cause of such statistical advance. In short, genetical selection is entailed by and measures but does not entail Darwinian selection.

In terms of the concepts used in the fundamental theorem, as strictly defined, therefore, the traditional theory of natural selection, that is, improvement in relation to environment necessitated by environmental pressures, has not been touched on at all. Increase or decrease in 'fitness' or if you like 'improvement' in Fisher's strict sense, is either the measure of the effect of Darwinian selection if there happens to have been any, or the measure of whatever else may have been happening to bring about increase in certain genetic factors rather than others. Similarly, what Fisher calls 'the progress of a population in fitness to survive' again means strictly no more than the increased probability of leaving posterity, whatever may be the reasons. There is no justification, in terms of the concepts 'as strictly defined', for considering this as 'progress' in any other sense.

7

Let us return now for a moment to Fisher's opening statement (at the beginning of Chapter 2) and put it alongside the fundamental theorem.

The improvement of any organism in relation to its environment is determined by its present condition. . . . The increase in fitness of any organism at any time is equal to its genetic variance in fitness at that time.

In the context of Fisher's argument, 'improvement', etc., clearly refers to the increased probability of leaving descendants, *for whatever reason*. And 'present condition' means present genetic variance in

fitness, that is, in the probability of leaving descendants. In other words, it means the summation of such probabilities itemised in the gene pool of the relevant population.

What does it mean, finally, to say that the second of these '*determines*' the first? The fundamental theorem asserts that the rate of increase in fitness of any organism at any time is exactly equal to the genetic variance in fitness at that time. But Fisher has just said (in his proof of the fundamental theorem) that the rate of increase in fitness here means rate of increase in fitness *due to all changes in gene ratio*.¹ Now the genetic variance in any measurement in population means, according to Fisher's earlier definition² the summation, for all genes, of the effects on *m* of the excess of any one gene over its alleles. But this is precisely the summation of the effect on *m* of all changes in gene ratio. To say that the rate of increase in fitness is due to changes in gene ratio is to assert a fundamental belief of modern genetic theory. To identify the increase so caused with the 'genetic variance in fitness' is to assert an identity. How can such a statement be said to 'determine' anything? We have present rate of increase in fitness = improvement; genetic variance in fitness = present condition. Either the second determines the first in the sense that we *are* simply stating an identity; and this tells us nothing about organic phenomena except as formalising what we already know—that, as Professor 'Espinasse has put it,³ wherever there are characters there are *some* genes that cause them. Or else the fundamental theorem is meant to direct our attention to the tables of reproduction which can be used to chart the trends in populations both at the level of individuals and at the genetic level, and to correlate trends in other measurements with trends in *m*. We could say, for example, that the overall increase in tallness of a population is equal to its genetic variance in tallness, because we believe that whatever trend a population is showing in respect to a given measurement has some genetic basis. To say that its overall trend in fitness is equal to its genetic variance in fitness is not to express any further 'determination' over and above this. Again, the only determination expressed here is the determination we know of from genetics, and the statistical measurements of *m* add nothing to this. There *may* of course be other causal connections in nature (in predator-prey situations and so on), and our statistical measurements

¹ Op. cit., p. 37

² Op. cit., pp. 30-32

³ In an address to the British Society for the Philosophy of Science in February 1959

may guide us in our analysis of, and perhaps even our search for, these. But in themselves statistics cannot specify such connections. In other words, the fundamental theorem is a guide to statistical technique which is overlaid on the causal relation of heredity and can be used as underpinning for the causal study of Darwinian selection; but in itself it asserts neither.

8

But how can the imposing edifice of modern neo-Darwinian theory rest on so narrow a base? In fact, it does not. It rests on the broader foundation of Darwinian thinking which is drawn into the circle of Fisher's statistical theory in virtue of the ambiguity of its central concepts.

We have only to look at Fisher's comments on the fundamental theorem to see how this works. He likens this principle to the second law of thermodynamics, as a statistical law that reigns supreme over a vast area of nature. But fitness, he says, though measured by a uniform method, 'is qualitatively different for every organism'.¹ Now this is not fitness in the sense of the mathematical chance of leaving descendants, which is a quantity, and cannot be 'qualitatively different' for each organism. On the contrary, Fisher is here referring to fitness in the sense which he goes on to amplify later in the same chapter, in the section on 'the benefit of the species': that is, fitness in the qualitative sense of an 'advantage' (of a particular kind) to the individual organism.² 'Fitness measured by *m*' will again become extremely important in the theory of the evolution of dominance (in chapter 3) where the biologist's concern is with the probability, for certain genetic factors, of leaving a remote posterity, even though these factors may lie deeply hidden in the present population, phenotypically considered. But here (in the latter part of Chapter 2) the statistical concept of fitness has been translated into old-fashioned Darwinian fitness: in terms of our previous example, the benefit to a Manchester moth of being black and the harm to his cousin of being speckled. Of course if mostly peppered moths are eaten and black ones spared, the population will come to include more black and fewer speckled individuals. But the statistical result to the population in the future and the immediate

¹ Op. cit., p. 39

² This section is added in the second edition, but what Fisher says here is implicit in the usage of the first edition also.

benefit to this sooty moth on this grimy tree trunk today are not the same thing. Yet through the identity of the word 'fitness', the insistence that selection has to do with present advantage, not 'trends', is here attached to Fisher's original, stricter concept of 'fitness', which thus becomes Darwinian as well as genetical and immediate as well as long-run. So we have genetical selection for later and Darwinian selection for now fused under a single word 'fitness'.

And 'progress' is equally elastic. We have 'progress' in fitness-measured-by-*m*: for example, progress in the height of the population if tall individuals are on the increase (never mind why); and at the same time we have progress in an advantageous character in the sense of increased adaptation, if, say, tallness (as in the proverbial giraffe) actually, for some reason, causes its possessor to get more to eat than his shorter brothers and therefore causes, rather than simply measuring, survival.

9

There is more to it than this, however. The advantage to the individual organism on which Fisher insists is not just the traditional one of an advantage in facing predators or the like, that is, as against our contemporaries, a lesser chance of death. It is also (and must be, in an evolutionary context) the chance of reproduction. 'It will be observed', Fisher writes,

that the principle of Natural Selection, in the form in which it has been stated in this chapter (i.e. the fundamental theorem) refers only to the variation among individuals (or co-operative communities), and to the progressive modification of structure or function only in so far as variations are *of advantage to the individual in respect to his chance of death or reproduction*. It thus affords a rational explanation of structures, reactions and instincts which can be recognised as profitable to their individual possessors. *It affords no corresponding explanation for any properties of animals or plants which, without being individually advantageous, are supposed to be of service to the species to which they belong.*¹

Now if we omit the qualification 'or reproduction', this is a fair statement of the situation described by Darwin. Where the benefit of individuals is concerned, it is the chance of death that selection controls. True, we may also infer that, since the chance of reproduction is obviously tied to the chance of death, the future numbers of organisms possessing immediately advantageous characters, given certain

¹ Op. cit., p. 49 (my italics)

environmental conditions, will increase in numbers also, if and when those conditions persist. Moths that are eaten cannot reproduce thereafter. But the *immediate advantage* to the moth in this case is not reproduction, but the omission of being eaten. Reproduction would be an advantage to the moth's descendants, and indirectly to the moth in so far as it is advantageous to it to satisfy its instincts; but in the situation of pure Darwinian selection—as in this case of the black moth on the black tree trunk—it is the character that keeps one alive that counts. Reproduction as the continuation of the species is a matter that is indirect and inferred; it is staying alive that is the immediate benefit.

Yet the 'or reproduction' alternative is essential to Fisher's *genetical* selection—and again, to the future-directed character, in particular, of the evolution of dominance: where in present-day recessives—which may some day become dominant—we are dealing, as he says himself, with the 'chance of leaving a remote posterity' by storing up characters that will some day be useful in some distant future environment. Such a chance is surely meaningless in terms of 'advantage to the individual' unless in some Biblical sense, that a man feels he is cut off from immortality if he is cut off from having living descendants. In short, we have tied together in the concept of fitness three kinds of 'selection': genetical selection, which is future-directed but only in a Pickwickian sense selection at all; Darwinian selection for now, directed to immediate advantage; and Darwinian selection for later, in reference to future advantage. The first specifies progress in the purely statistical sense of increased chance of survival of some genes rather than others; the second in the sense of short-run increase in adaptation; the third, long-run increase in adaptation.

Finally, added to these, we must notice another of Fisher's comparisons between his theorem and the Second Law. While physical systems run downhill, he says, evolution tends on the whole to produce 'progressively higher organisation of the organic world'.¹ This is progress in a new sense, and one which escapes all Darwinian considerations, though again its existence, once admitted on other grounds, might be recorded with the help of statistical methods.

Thus the larger conceptual structure of the theory is both richer and less exact than the proof of the fundamental theorem would lead us to expect. It consists of a network of concepts, in which statistical and deterministic, genetical and Darwinian meanings, short-run and long-run assessments, reinforce one another in a self-confirming circle.

¹ Op. cit., p. 40

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For if fitness means, after all, advantage to the individual, then the long-run trends of reproduction tables *must* represent real benefits in each generation, and if at the same time fitness *means* the statistical progression measured by m , then such progression must entail the whole causal context that Darwinian 'fitness' implies. It is only in this way that the 'determination' mentioned in Fisher's initial thesis can be said to make sense. And similarly, the references to internal or genetical and external or Darwinian 'selection' and the references to future (genetical or Darwinian) and present (Darwinian) 'fitnesses' confirm one another, in circular re-inforcement, indefinitely.

10

This ambiguity will become clearer if we pin-point the narrower and wider senses in which the concept of 'improvement' occurs in the argument and consider their evolutionary import.

First, there is the strict statistical meaning of the Malthusian parameter: the increase in the probability that organisms possessing certain characters will leave offspring. Such an increase, though calculated for an infinitesimal period, has meaning only over time. What it asserts about organisms in time may be taken in two ways: either as a tautological statement: what has survived has survived, what is on its way to surviving is on its way to surviving; or as the retrospective appraisal of an achievement: what has survived is not what has failed to survive, but what has succeeded. But this says nothing of *why* that might have happened, either in terms of adaptive relations to environment or anything else. Tables of reproductive value record the multiplication of some characters and the disappearance of others; that is all one can say in these terms. And in terms of these tables, one may assert the probable increase of some genes rather than others.

In this sense of 'improvement' Fisher is right in saying that organisms must show 'improvement' up to the moment of extinction. In terms of the increase of some genes as against their alleles, they doubtless do. For since the sum of any gene and its alleles in any population always = 1, we can always take the increasing rather than the decreasing gene (or genes, where there are multiple alleles) and so get a picture of 'improvement'. This is really why, in the fundamental theorem, the calculated value is always positive. Not only is the time interval, dt , necessarily positive, but the changing gene ratios also can always be taken in this sense.

Alongside this statistical formulation, however, consider what Fisher says about the universality of 'improvement' in his comment on the fundamental theorem. His first comparison with thermodynamics is to this effect:

The systems considered in thermodynamics are permanent; species on the contrary are liable to extinction, although biological improvement must be expected to occur up to the end of their existence.¹

The phrase 'biological improvement' brings us back to the 'improvement in relation to environment' from which we started; it is what Fisher describes elsewhere as 'progress determined by Natural Selection', the full Darwinian improvement so emphatically underlined in the second edition. According to this conception, organisms are in fact constantly becoming better adapted to their niches in nature, as natural selection weeds out the imperfectly adapted; and therefore it follows, further, as Fisher in fact argues in some detail, that the explication of all cases of extinction must be referred to deterioration of the environment. Thus when the dinosaurs died out, for example, something must have gone wrong in their environment to bring this about. That this is actually so in nature, however, cannot be inferred from the fundamental theorem by taking 'improvement' in its strict and statistical sense.

We are dealing here, in other words, with a second concept of improvement. We are asserting that there is improvement in the sense of the appearance of adaptive relations with immediate effectiveness here and now, characters or functions which are genuinely advantageous in relation to their bearer's actual environment, and which may therefore, as compared with their absence, be called 'improvements'. These are the improvements effected by Darwinian selection, and the concepts presupposed in the assertion of such improvement are not by any means the same as those supporting the statistical concept. While statistical 'improvement' entails the conception of the organism as an aggregate of gene effects, Darwinian improvement entails in addition the conception of the organism as a machine with parts adapted to the performance of their special functions. The improvement that is effected here is that of increasing adaptation in the sense of

¹ Op. cit., p. 40. For an excellent discussion of the logical place of biological improvement in Darwinian and neo-Darwinian theory, see the article by Flew mentioned in note 2, p. 30 above.

specialisation, of fitting in better and better to a special niche in nature. The evolutionary import of such improvement is *katagenetic*, to use a term introduced by the German evolutionist Rensch. That is, it is evolution downhill, in the sense that species break up into varieties and subspecies as specialised demands are made on them. Darwin's finches are a classic example.

As against genetical 'improvement', which is meaningful only as an assessment of a trend over a lapse of time, this kind of improvement is short-run. On the other hand, it is not intelligible, as the statistical concept purports to be, in terms of gene ratios alone. Its assessment depends on the recognition of phenotypes, as wholes, not as aggregates of genes, and on the recognition of the relation of such wholes to their environment, to predators, to climate, and so on and so forth. It is neither future moths, nor gene pools, to whom it is advantageous not to be taken by a bird, but this black moth on this tree trunk today. And the evolutionary trend which establishes and maintains a phenomenon like melanism expresses the accumulation of millions of such individual escapes and individual disasters, not to genes or gene pools, but to moths, whether today or yesterday or (from some cause other than the industrial revolution) a million years ago.

12

So much for our first pair of 'improvements'. However, as critics of natural selection theory have long been saying, there also seem to be adaptive relations which develop only slowly, and which do not appear to benefit their possessors at the beginning of their development. Such long-run adaptation appears to entail a third meaning of 'improvement'. It refers to characters or functions which will be 'better' for future phenotypes in future environments. Such improvement we may call *quasi-anagenetic*; it still concerns particular adaptive relationships and specialisations, but specialisations which accompany the emergence of new forms (rather than new sub-styles of old forms) and which develop slowly over a very long period of time. Now this seems to be the kind of situation which Fisher explicitly excludes, exiling the 'benefit of the species' approach as teleological and irrational. In terms of what he says there, one would conclude that all ultimately useful characters must have been in some direct way useful even in their minute beginnings. And it is true that this has been shown to be possible or even likely in a number of cases, for instance, primitive

photoreceptors, electric organs, feathers, and so on. One can, if one likes, extrapolate such instances to all cases of clearly adaptive characters. Yet the forward reference, as distinct from immediate utility, seems essential to Fisher's own type of statistical selection, and in particular to changes in the relations of recessive and dominant alleles. Here we have recessives, either useless or even harmful, hidden away in a population over a long period of time—up to the moment of a new environmental situation which makes them advantageous and so calls forth modifiers that turn them into dominants. Only the reference to 'remote posterity' makes sense of this story. True, there is the case of sickle-cell anaemia: where we have a gene which is lethal in the homozygote, but in the heterozygote actually gives protection against malaria, and is kept going in the population by this beneficial effect. Supposing a situation in which sickling were no longer harmful, we could imagine that a character now maintained, but in a recessive state, because of the advantageous nature of the heterozygote, might become dominant and pervade the population much more completely. And we might then extrapolate this kind of process to all cases of the retention of recessives apparently for the sake of their future usefulness, but really for the sake of some other present benefit. Yet even then, the modifiers which will eventually make the now-recessives dominant must be lurking in the population ready to leap into action when the environment demands, and thus the future-directedness of the whole procedure is simply transferred to them. It seems strange to say, as Darlington does, that the genes are endowed with an automatic property of foresight,¹ yet the reference to 'remote posterity' which dominates Fisher's argument on dominance does indeed suggest some such idea.

13

How can Fisher and other evolutionists who follow him remain so happily unaware of this third and uncomfortably long-term 'improvement'? What happens, I think, is something like this. We have so far, three concepts of 'improvement'. *Improvement 1, statistical selection*, is the measure of *improvement 3, long-run adaptation*. But long-run adaptation is *adaptation*, and where there is adaptation there must, in the light of the Darwinian theory of natural selection, be *improvement 2, Darwinian selection*—for Darwin has proved that that is how adaptation is produced. So improvement 3, which is in fact

¹ C. D. Darlington, *Evolution of Genetic Systems*, 2nd edn., Edinburgh, 1958, p. 239

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unintelligible in terms of the dictum of improvement 2 (since in its terms all adaptation bears on immediate, not remote benefit) is nevertheless subsumed under it through the measurement of both of them by the techniques of improvement 1. Moreover, improvement 1 is expressed by a differential, which can be interpreted as a summation of gene changes *now*—short-run trends—or over as long an interval as you like—long-term trends; but at the same time, since it is one statement (the fundamental theorem) it must express one relationship, and since in Darwinian terms improvement 3 would be nonsense, this one relationship must be the situation covered by improvement 2.

The way in which 1, 2 and 3 are assimilated to one another is most evident if we place the argument about the fraction $1/n$ and the efficacy of very slight 'selective intensities' alongside the argument about immediate advantage. As Fisher himself argues, irregular variation in the rate of increase in fitness as measured by m is more apparent in a single generation than over a longer period, and therefore very small 'selective values' are sufficiently strong to establish themselves *over a long enough time*. This statistical observation is often used against those who object to natural selection theory because of the difficulty of accounting for the first beginnings of what will ultimately be useful traits. Selection can do so much *because it has so long*.¹ Yet if one speaks of trends in evolution, of orthogenesis or the like, one is told that this is nonsense because evolutionary modification always consists in the selection of the immediately useful at each step, in each generation.² So improvement 3, long-run adaptation, with its statistical measure in improvement 1, is used to answer one objection, while improvement 2, which can also be expressed by the same statistics, applied to short-run situations, is invoked to answer another.

14

Finally, we must mention a fourth meaning of improvement, the kind of improvement which is truly an advance to higher forms of life. Such improvement is explicitly mentioned only in Fisher's remark about his fundamental theorem and the tendency to 'higher organisation' (as contrasted with the Second Law and entropy). This is the only kind of improvement which represents true *anagenesis* in the

¹ See, for example, the contributions of Huxley and Fisher to the Huxley, Hardy, Ford volume, referred to in note 2, p. 25 above, or Huxley's *Evolution in Action*, 1953.

² Loc. cit.

sense of emergence, or the appearance of genuine novelty at a higher level of richness or complexity. But this is also a kind of improvement which neither statistical genetics nor selectionist biology can handle, since it is neither quantitative nor adaptive. It is best for selectionists to ignore it, as Darwin warned himself to do when he wrote in his copy of the *Vestiges* 'Never speak of higher or lower in evolution'. Yet the great outlines of the fossil record are there, and demand to be spoken of, especially since the fact that we can speak of them is one of the surprising results of the process they record. But evolution as macro-evolution, as the emergence of life and of higher forms of life, outruns both the concept of gene-substitution, and of improvement in relation to environment. It makes sense only as an achievement—an achievement for which statistical methods can measure the necessary, but not the sufficient conditions.

15

One brief concluding remark. I have side-stepped here altogether the question of prediction and retrospect, of the historical nature of evolutionary explanation: a question which is very close to the philosophical difficulties raised by Fisher's theory. Evolutionary theory is essentially an assessment of the past. Fisher treats it in terms of present and future. Just how closely the philosophical confusions of this kind of argument are related to the attempt to think unhistorically about an historical subject matter, I should not at the moment venture to say.

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A CAUSAL CALCULUS (II) *

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6 Two-state Markov Processes

The radioactive process described in Axiom 23 can be slightly generalised by permitting return from the black to the white state, with a parameter β corresponding to the α of the white-to-black transition. We have a two-state Markov process with continuous time. The parameters α and β are of course both non-negative. In the special case of the radioactive particle we have $\beta = 0$.

It can be shown that

$$Q(E : F) = \log ((\alpha + \beta e^{-(\alpha+\beta)T})/(\alpha - \alpha e^{-(\alpha+\beta)T})).$$

If the particle ever entered the black state during the time interval, T , the chain would be cut and the degree of causality would be zero. Assuming that this does not happen, we can calculate $\chi(E : F)$ by applying a Riemann dissection to the interval, so as to obtain a causal chain consisting of a finite number of events, and then proceed to the limit as the fineness of the dissection tends to zero. By applying T17 and A9 we find that

$$\chi(E : F) = - \log (1 - e^{-\alpha T}),$$

which is mathematically independent of β .

For large T , both Q and χ are exponentially small, but Q is smaller than χ , and is much smaller if β is large. This is reasonable since, if β is large, the initial state makes little difference to the probability of being in the white state at the end of the interval.

Note that χ is the degree to which being in the white state rather than in the black state at the end of the interval was caused by being in the white state rather than in the black state at the start of the interval. A similar explicit description can of course be given for Q .

7 Partially Spurious Correlation

A well known pitfall in statistics is to imagine that a statistically significant correlation or association is necessarily indicative of a causal relationship. The seeing of lightning is not a cause of the hearing of

* The first part of this article appeared in the previous Number.

thunder, though the two are strongly associated. Such associations and correlations are often described as 'spurious', a better description than 'illusory'. They may also be *partially spurious*, and the explicata for Q and χ should help with the analysis of such things. Smoke and dust might be a strong cause of lung cancer, but smoking only a weak cause. Even so, the correlation between smoking and lung cancer may be high if there is more smoking per head in smoky districts. I mention this only as an example, and have not made a special study of this problem.

Note that

$$Q(E : F \cdot G/\bar{F} \cdot \bar{G}) = Q(E : G/\bar{F}) + Q(E : F|G),$$

so that the tendency to cause can be split into components, somewhat in the manner of an analysis of variance. For example, the tendency for lung cancer to be caused by smoking and living in a smoky district as against not smoking and living in a clean district is equal to the tendency through living in a smoky district, given no smoking, plus the tendency through smoking, given that the district is smoky. It is also equal to the causal tendency through living in a smoky district, given that one smokes, plus the tendency through smoking, given that the district is clean. This approach to the analysis of spurious correlation is entirely different from, and more quantitative, than the approach used by Simon.¹

Let

$$K(E : F) = -I(\bar{E} : F),$$

the *intrinsic* causal tendency of E by F . It is related to Q in essentially the same way that I is related to W , since

$$\begin{aligned} Q(E : F) &= K(E : F) - K(E : \bar{F}), \\ Q(E : F/F') &= K(E : F) - K(E : F'). \end{aligned}$$

K does not depend on the negation of F , so its use enables us to avoid the distribution, D , of Section 4. We have

$$K(E : F \cdot G) = K(E : F) + K(E : G | F),$$

so that K can be split up into contributions from various sources in a simpler manner than Q . In my opinion both K and Q will probably have useful applications in statistics and physics.

The remainder of this paper is primarily concerned with the extension of the explication of causal strength to general nets, in order that degree of causality should be generally explicated. The next section however contains a formal definition of a causal chain, which strictly

¹ Herbert A. Simon, *Models of Man*, New York and London 1957

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was required in what has already been discussed. I postponed it in order not to interrupt the thread of the argument.

8 *Causal Chains*¹

Let $F = F_0, F_1, \dots, F_{n-1}, F_n = E$, be $n + 1$ events such that (for $i = 0, 1, \dots, n - 1$):

(i) F_i and F_{i+1} are contiguous in space and time, or approximately so.

(ii) No two of the events overlap much in space *and* time.

(iii) All the events occurred (or will have occurred, i.e. they 'obtain' but I prefer to write simply 'occurred').

(iv) F_{i+1} started later than F_i did.

(v) F_i had a positive tendency to cause F_{i+1} .

(vi) If F_i is given, then the probability of F_{i+1} is unchanged if one or more of the earlier events did not occur, i.e. we have a Markov chain.

(vii) If the chain is embedded in a completely detailed chain containing intermediate events, then condition (v) will remain true for the more detailed chain.

Then we say that F_0, F_1, \dots, F_n or $F_0 \rightarrow F_1 \rightarrow \dots \rightarrow F_n$ is a causal chain connecting F to E . Perhaps it should be called a 'putative causal chain' if condition (vii) has not been established. In practice all causal chains are putative, but there are degrees of putativity.

The failure of condition (v) may be said to 'cut the chain'.

A causal net will be formally defined in Section 11. A chain is a special case of a net.

9 *Independent Causal Tendencies*

Let G_1, G_2, \dots, G_m be independent given H , and also independent given $H \cdot \bar{E}$. Then it is easily proved, with the help of T15, that the tendencies to cause E are additive in the sense of the theorem below. It therefore seems reasonable to say in these circumstances that the G 's have *independent tendencies to cause E given H* . The events G_1, G_2 , and G_3 of A20 exemplify this definition, with $H = F$, and also with $H = \bar{F}$; that they are independent given \bar{E} is trivial since their probabilities are then all zero.

¹ Cf. Hans Reichenbach, *The Direction of Time*, Berkeley and Los Angeles 1956, Index reference under 'Causal chain', and *The Philosophy of Space and Time*, New York and London 1956, Index reference under 'Casual chain' [sic]

T18. If G_1, \dots, G_m have independent tendencies to cause E given H , then

$$Q(E : G_1 . G_2 . \dots . G_m | H) = \sum_i Q(E : G_i | H).$$

The nets of A20 and T10 also exemplify the following definition :

A *bundle of parallel independent causal chains* from F to E is a class of chains from F to E such that, apart from F and E , each event on each chain is, given F and given \bar{F} , probabilistically independent of any collection of events on other chains, and also such that the penultimate events have independent tendencies to cause E , given their pasts.

10 Series-parallel Networks

As an extension of T11 it is natural to define the strength of a bundle of independent causal chains as the sum of the strengths of the individual chains.

For a 'chain of bundles', in a self-explanatory sense, we can first calculate the resistance by summing the resistances of the individual bundles, and then obtain the strength from T16. We can extend the process to bundles of chains of bundles and so on. In other words we can construct natural rules for evaluating the causal strength of any 'series-parallel' net. Topologically these are the same as the two-terminal series-parallel networks whose enumeration was considered by MacMahon.¹ Not all networks are of this type.

11 Causal nets 'Having Independence'

Let π be a class of events all of which occurred. For each event, G , in π , there is a subclass of earlier events, G_1, G_2, \dots, G_k , which so to speak, 'lead in' to G . By 'lead in' is meant that the probability of G , given which of G_1, G_2, \dots, G_k occurred and which did not, is independent of any further assumptions of which other events in π , earlier than G , occurred. (Note that not all the events in π are regarded as 'given' even though they all actually occurred. This should cause neither surprise nor confusion to those who are familiar with the idea of a conditional probability.) We may think of k oriented links joining G_1, G_2, \dots, G_k to G . If the whole class, π , is connected together by means of such links we describe π as a *causal net*. If E is the

¹ P. A. MacMahon, 'The combination of resistances', *The Electrician*, 1892, 28, 601-602. Or see John Riordan, *An Introduction to Combinatorial Analysis*, New York and London 1958, pp. 139-143.

latest of the events in the net, and can be reached from each other event by passing through a succession of links in the right direction, then the causal net will be said to *lead to* E. If F is the earliest of the events in n, and each other event can be reached from F by passing through a succession of links in the right direction, then the causal net will be said to *lead from* F. If both conditions are satisfied, the net will be said to *lead from* F to E. For example, a net leading to E could have the form of a 'tree', but a net leading from F to E could be a tree only if it were a chain.

In this definition we may call G_1, G_2, \dots, G_k the *immediate predecessors* of G. A causal net will be said to *have independence* if, for each G in the net, the immediate predecessors have independent tendencies to cause G given the past.

For each link $G_i \rightarrow G$, having a 'p' and a 'q',

$$p = p_i = P(G | G_i), q = q_i = P(G | \bar{G}_i),$$

let the *quasiprobability*, π , be defined as

$$\pi \doteq \left[\frac{p - q}{1 - q} \right],$$

in which the 'square' brackets indicate that $\pi = 0$ if $q \geq p$. The quasiprobability reduces to p when $q = 0$. We know from T17 that the quasiprobabilities are multiplicative for a chain, and the strength of the chain is the same as if the quasiprobabilities were ordinary probabilities and the q 's were all zero. Also, from T15, we have

$$S(p, q) = -\log(1 - \pi),$$

so that for a bundle of the type occurring in T10 the quasiprobabilities again behave like probabilities, in view of the additivity of the strengths of the chains.¹

Let us now consider an arbitrary finite causal net having independence and leading from F to E. We should like a general procedure for defining the strength of such a net that will include the results for the nets already considered, and which is simple, and which does not lead to a contradiction. I believe that the procedure illustrated in the following example satisfies these conditions. It would of course be more satisfactory if some convincing axioms could be laid down that would uniquely determine the procedure.

¹ The term 'pseudoprobabilities' would conveniently refer (by analogy with the pseudo-random numbers that are often used in Monte Carlo calculations) to the apparent probabilities that occur in a deterministic, but pseudo-indeterministic, set-up.

In the diagram, the quasiprobabilities $\pi_1, \pi_2, \dots, \pi_8$ are assigned, and pertain to the links of the net. It will be easier to appreciate the example if the π 's are at first thought of as ordinary probabilities (with all the q 's equal to 0).

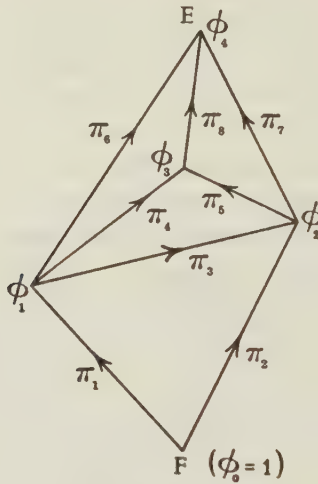


FIG. 2

The ϕ 's may be thought of as quasiprobabilities of the events. They are defined successively as follows :

$$\begin{aligned}\phi_0 &= 1. \quad \phi_1 = \pi_1. \\ \phi_2 &= 1 - (1 - \pi_2)(1 - \phi_1\pi_3). \\ \phi_3 &= 1 - (1 - \phi_1\pi_4)(1 - \phi_2\pi_5). \\ \phi_4 &= 1 - (1 - \phi_1\pi_6)(1 - \phi_2\pi_7)(1 - \phi_3\pi_8). \\ S(n) &= Q(\phi_4, 0) = -\log(1 - \phi_4).\end{aligned}$$

The reader should perhaps check that this procedure contains the previous ones as special cases.

12 Causal Nets in General

It will often be possible to divide up a time-slice preceding E into non-overlapping events whose causal influences on E are approximately but not absolutely independent. Let such a dissection of the time-slice be F_1, F_2, \dots, F_m . We need a definition of the strength of the causal link $F_1 \rightarrow E$ that will reduce to the value given previously in the case where F_1 and F_2, F_3, \dots, F_m are *causally independent* with respect to E, in the same sense as that defined above for nets having

independence. A simple definition having the required property is

$$S(E : F_1) = \log \frac{P(\bar{E} | \bar{F}_1 \cdot F_2 \cdot \dots \cdot F_m)}{P(\bar{E} | F_1 \cdot F_2 \cdot \dots \cdot F_m)} = W(\bar{F}_1 : \bar{E} | F_2 \cdot F_3 \cdot \dots \cdot F_m).$$

This definition reduces to the previous use of the expression $S(E : F)$ in the case of causal independence. But the strengths of the lead-ins do not add up to $S(E : F_1 \cdot \dots \cdot F_m)$ unless the F 's do have independent causal influences on E . We can cope with this difficulty by the introduction of 'interaction terms' in a sense analogous to the use of this expression in the literature of the design of statistical experiments.¹

We can think of an extra node in the causal net leading to E corresponding to every subset of the events F_1, F_2, \dots, F_m . For example, there will be a node corresponding to the pair $(F_1 \cdot F_2)$. The strength of the link from the node $(F_1 \cdot F_2)$ to E will then be taken as the 'interaction' term

$$t_{12} = s_{12} - s_1 - s_2,$$

where

$$\begin{aligned} s_{12} &= \log \frac{P(\bar{E} | \bar{F}_1 \cdot \bar{F}_2 \cdot \dots \cdot F_m)}{P(\bar{E} | F_1 \cdot F_2 \cdot \dots \cdot F_m)} \\ &= W(\bar{F}_1 \cdot \bar{F}_2 : \bar{E} | F_3 \cdot \dots \cdot F_m). \end{aligned}$$

When F_1 and F_2 are independent causes of E , we have $s_{12} = s_1 + s_2$, and the second order interaction term vanishes. The strength of the link to $(F_1 \cdot F_2)$, from an earlier event, G , is

$$W(\bar{G} : \bar{F}_1 \cdot \bar{F}_2 | G_1 \cdot G_2 \cdot \dots),$$

where G_1, G_2, \dots are the other immediate predecessors of F_1 and F_2 . The definitions of the s 's are forced, if we regard conjunctions of the F 's as single events. An example of a third-order interaction is

$$\begin{aligned} t_{1234} &= s_{1234} - s_{234} - s_{134} - s_{124} - s_{123} + s_{12} + s_{13} + \\ &\quad \dots + s_{34} - s_1 - s_2 - s_3 - s_4, \end{aligned}$$

where the notation is now self-explanatory. In any piece of causal analysis one would try to choose the dissection of the time-slice so as to make the high-order interactions negligible.

Since

$$s_{123 \dots m} = \sum s_i + \sum t_{ij} + \sum t_{ijk} + \dots,$$

our enlarged causal net has the property of additivity of strengths of

¹ See, for example, *Design and Analysis of Industrial Experiments*, ed. by O. L. Davies, London and Edinburgh 1954; index reference under 'Interaction'.

lead-ins that we previously had for causally independent lead-ins. It is therefore now potentially possible to apply the method of Section 11 to define the causal strength of an arbitrary finite net from F to E.

13 *Degrees of Causation*

We may now define $\chi(E : F)$ as the limit of the strength of the net joining F to E and containing all intermediate events, when the events are made smaller and smaller. I have not proved that this limit exists. The proof, if possible, would depend on a physical theory, and would be mathematically intricate. Note the implication : whether degrees of causality exist is a matter of physics, even if we take for granted that physical probabilities exist.

In practice one must always over-simplify or simplify in order to be able to judge, estimate, or guess, the value of $\chi(E : F)$. (In the past, χ has been given only a few values, such as 'small', 'moderate', and 'large'.) There is always the possibility that something has been overlooked. Even in a statistical experiment involving randomisation, from which we can apparently deduce that some $\chi(E : F)$ is large, in fact E and F may both have been caused by some preceding event. The table of random numbers might have been seen by the famous lady tea-taster,¹ or there may have been some psychokinesis. We are always thrown back on judgment.

14 *Big Events*

So far the analysis has assumed F and E to be small events. If F is big we may imagine it split up into many small events, and imagine all these to be 'short-circuited' from an earlier 'input node'. By 'short-circuited' is meant that the resistances of all the imaginary links are taken to be zero. We may apply a similar process to a big E by short-circuiting its small parts to a future output node. The previous methods may then be applied even if F does not end before E begins.

Appendix I. Correction of some errors in previous work

Reichenbach² says that F is causally relevant to E if $P(E | F) > P(E)$ and if there is no set of events earlier or simultaneous with F that 'screens off' E from F. By 'screens off' he means that the probability of E given these

¹ Sir Ronald A. Fisher, *The Design of Experiments*, 5th edn., Edinburgh and London 1949, Chapter 2

² Page 204 of the first reference in footnote 1, p. 45 above

other events is unchanged if F is also given. The property is analogous to the Markov property.

It seems to me that this definition is not acceptable as it stands for much the same reason that my previous paper is not acceptable. For let G be any set of events earlier than or simultaneous with F . G might be some exceedingly biased selection of individual molecules, such as those that are proceeding south at a thousand miles per hour. Consider the expression $P(E | G) - P(E | G \cdot F)$. Normally this will be positive for some G , say G_1 , and negative for some G , say G_2 . We now imagine G_1 to be gradually distorted into G_2 . The above expression must change sign at some point during this gradual distortion, at which 'time' its value will be zero. Hence the second part of Reichenbach's definition seems to be vacuous. In order to patch up the definition it seems to be necessary to take G as the complete state of the universe at the time F started.

In my previous paper, conditions C_7 to C_{10} were vacuous for much the same reason, though it may be possible to patch the thing up, as stated therein (inserted in proof), by insisting that G should be in some sense a 'natural' event.

Appendix II. The meaning of 'state' in quantum mechanics (see Section 4)

The seven relevant interpretations of 'state' in quantum mechanics are the first seven on the following list. All seven of these meanings, and perhaps others, should be taken into account in a comprehensive discussion of the place of probabilistic causality in quantum mechanics.

(i) The class of all past phenomena, classically describable. (ii) The class of phenomena extending only a short way into the past. (iii) The wave function of a physical system, under observation by another physical system. (iv) The joint wave function of the pair of systems. (v) The wave function of one system conditional on an assumed wave function of another system. This is the 'relative state' of Hugh Everett III, 'Relative state formulation of quantum mechanics', *Rev. Modern Physics*, 1957, 29, 454-462. (vi) The wave function of the entire universe if this has any meaning. See Everett, *loc. cit.* (vii) The wave function of the entire universe together with all other past phenomena. (viii) An ensemble of wave functions. See, for this eighth interpretation, R. C. Tolman, *The Principles of Statistical Mechanics*, Oxford 1938, Section 98.

(concluded)

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COMMUNICATIONAL EPISTEMOLOGY (II) *

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2 *Correction of Misunderstandings*

OUR inquiry may be divided into the following questions:

- (1) How does A find out B's thinking pattern, encoding and decoding functions?
- (2) How does A know that his interpretation of B's behaviour is correct? How does A check the correctness of his interpretation?
- (3) How does A know that B's interpretation of A's behaviour is correct? How does A check the correctness of B's interpretation?
- (4) How does A become aware of the incorrectness of his interpretation of another person's behaviour?
- (5) How does A know that B has become aware of B's misinterpretation of A's behaviour?
- (6) What do people do to remove the detected misunderstandings? Do they employ a successful method? Do they employ the best method?
- (7) What are successful methods and the best methods for eliminating misunderstandings?

The first question is essentially what is known to communication engineers and behavioural scientists as the problem of the black box. A black box is a transducer (or a processing system) which processes a given input into an output different from the input. The processing operation inside the black box is unknown. It is not open for inspection. All one can do is to examine the relationships between various forms of the input and their related outputs. One has to infer the nature of the operation inside the box from these relationships. A classical example of the black box is the animal used in stimulus-response studies in psychology. Communication engineers and

* Part I appeared in the previous Number.

mathematicians have elaborated refined theories of the solution of the black box problem.

A person as an epistemological object is a black box both to other persons, and, to some extent, to himself because much of his internal thought processes and 'state algebra' is at the level of the unconscious. For this reason, communicational epistemology should include the study of the internal communication of the individual, though here we limit ourselves to discussion of interpersonal communication. Any other person, especially a new acquaintance or some one from a foreign culture, is more or less a black box. He is a triple transducer, in which three transducers are connected in series—the decoder, stal-operator and the encoder. The output of the decoder does not come out of the black box, but goes directly to the input of the stal-operator. Likewise the output of the stal-operator becomes the input of the encoder, and only the output of the encoder comes out of the black box. Another complication is that the person learns and unlearns, i.e. changes his stal, encoding and decoding functions, and so the properties of the black box transducers are not constant. And so Question 1 is a very complex black box problem. But, as long as it is a black box problem, it is possible to formulate it mathematically. Some existing theories partly solve the problem. The problem of a stal without encoder and decoder has already been solved except for some initial states.¹

For reasons to be given later, there is no way of affirming the correctness of one's interpretation of others. There is, however, a way of detecting an incorrect interpretation. It is provided by the criterion of inconsistency, not of inconsistency between one's interpretation and another person's statement or action, but of inconsistency *within* one's interpretation. One has no access to another person's statement or action except through one's decoding. One may ask another person whether one's interpretation is correct. But the question has to be decoded by this other person, and the answer is first coded by this other person and then decoded by oneself. Anthropologists often find that the questions put to persons within a culture about the culture are often irrelevant or meaningless to the persons asked, not for linguistic or semantic reasons, but because of cultural differences; and the answers, if answers are insisted upon, are meaningless or do not answer the question.

One becomes aware of the incorrectness of one's interpretations when one discovers inconsistencies within them. But it may happen

¹ C. E. Shannon (editor), *Automata Studies*, see especially the article by Moore

that the correct interpretation seems strange or inconsistent to the interpreter because he is not used to interpretation of that type. In such a case, he at least discovers that there are differences between his own thinking pattern and that of the other. For example, should a Chinese philosopher say: 'In Chinese philosophy the concept of substance does not exist, and the subject-predicate relationship is dispensable',¹ the interpretation may be correct according to the Chinese pattern of thinking, but this correct interpretation is unacceptable to most Western philosophers who base their logic on the concept of substance and the subject-predicate relationship. This point will be further discussed under 'dimension reduction' and 'projection'. A further example is provided by the important rôle played by the concept of the unconscious in psychoanalytic and psychiatric thinking. For many mathematicians and physicists the concept of the unconscious is unacceptable and is inconsistent with their thinking. Thus, an inconsistency within one's interpretation of another person's behaviour does not necessarily mean that the interpretation is incorrect. It means one or both of two things: (1) One's interpretation is incorrect; (2) One has to change the thinking pattern in terms of which the interpretation appears inconsistent.

Let us restate the preceding discussion more formally. Let the situation be that a person R interprets another person T. The transmitter of the communication, T, encodes his thoughts or state of mind into conscious or unconscious, verbal or nonverbal behaviour. The receiver, R, decodes T's communication consciously or unconsciously.

Let f be the encoding function of T, f_c and f_u the parts of f pertaining to conscious and unconscious encoding respectively. Let T's decoding function be the inverse of f , denoted by f^{-1} . Let g , g_c , g_u be similarly the encoding functions of R, and g^{-1} the inverse of g . Let f , g , f^{-1} , g^{-1} be univalent functions. Let x represent the internal state of T and let x_i represent different values of x . (The subscript i need not be a denumerable ordinal nor even a member of an ordered set, but the axiom of choice has to be assumed.) It should be noted that $f_c(x)$ does not necessarily have to be decoded by g_c^{-1} : it may be decoded by g_u^{-1} . Similarly, $f_u(x)$ may be decoded by either g_u^{-1} or g_c^{-1} .

At the beginning of the interaction between T and R, nothing about f is known to R, and nothing about g is known to T. The functions f_c and g_c can be adjusted during the interaction by the process

¹ T. S. Chang, 'A Chinese Philosopher's Theory of Knowledge', *ETC.: A Review of General Semantics*, 1952, 9, 203-226

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of approximation in order to give maximum understanding or maximum misunderstanding, depending on whether the purpose of the communication is understanding or deception, forming a non-zero sum two-person game of 'evolving' strategies—to be defined later. The function f_u cannot be adjusted consciously by T, and R has to learn to adjust g^{-1} to f_u by experience. Similarly g_u^{-1} cannot be adjusted by R consciously, and T has to learn to adjust f to g_u^{-1} . This one-sided adjustment is a non-zero sum one-person game of evolving strategies. The only criterion R may use for determining whether $g^{-1}f(x)$ is equal to x or not is the consistency or inconsistency of the set of $g^{-1}f(x)$ in the thinking pattern of R which we will denote by J. Symbolically,

$$J\left(\begin{smallmatrix} S \\ i \end{smallmatrix}\right)g^{-1}f(x_i) \in C$$

where $\left(\begin{smallmatrix} S \\ i \end{smallmatrix}\right)$ is the set when x_i runs through all values of x , ϵ is elementhood relationship, and C is the set of all consistent thinkings. If this is not satisfied, J and g can be gradually adjusted to give a maximum consistency:

$$\text{Max Con } J_m\left(\begin{smallmatrix} S \\ i \end{smallmatrix}\right)g_n^{-1}f(x_i) \\ m, n$$

The consistency criterion ensures that the interpretation of R is consistent, but not that it is correct. There may be self-consistent but mutually contradictory interpretations. Though R may succeed in constructing several different consistent interpretations he may not find the right one, i.e. the consistency criterion does not guarantee that $x_i = g_n^{-1}f(x_i)$. It does not help, in order to check whether R's interpretation is correct, to feed $g^{-1}f(x)$ directly back to T, because it will only give $f^{-1}gg^{-1}f(x)$, which is x , since f , g and their inverse functions are univalent. If a man thinks that π is an irrational number and says ' π is an irrational number ' to a parrot, then if the parrot replies ' π is an irrational number ' and the man 'understands' what the parrot says and agrees with it, it does not guarantee that the parrot's decoding of the man's message is the same as the man's. Similarly, a professor of philosophy may make a statement to a student. The student interprets the statement in his own way and expresses his interpretation by making exactly the same statement as the professor's. And the professor replies: ' That is right.' But this does not prove that the professor's thought and the student's interpretation of the professor's statement are the same. A better check is to make the

student reformulate the statement in other words, or to make him apply it to different examples. The reformulation and the application of the statement to examples are intellectual operations. If we make a 'stal' complex enough, we can include such intellectual operations in the 'stal'.

If the student reformulates the given statement in the same way as the professor would, or if he gives the same kinds of examples as the professor would, then the professor thinks that the probability that the student understood the statement is greater than otherwise. Similarly, we can make R process his interpretation and express the result of his processing. For example, if T transmits the word 'bird' by telephone and lets R spell out 'b . . . i . . . r . . . d'; or transmits the numbers 3 and 5 and lets R add them and feed back the result; or transmits 'Your father does not approve your choice of career' and lets R figure out the solution and reply, then all these replies provide some check on R's interpretation of T's first statement. Symbolically,

$$f^{-1}g P_R g^{-1}f(x)$$

where P_R is the process by R, which may or may not be equal to P_T , where P_T is the process by T. If we find that it is equal for all P_R 's (and for corresponding P_T 's) and for all x_i , then for the practical purposes of life we may assume that $g^{-1}f(x)$ is equal to x , because on the behavioural level we cannot detect the differences between f and g . Here is a way of formulating behaviourism in terms of the mathematics of communicational epistemology. It should be emphasised, however, that communicational epistemology is not a form of behaviourism—a point which will be discussed at a later opportunity.

We can take a further step. Let the set of all P_R 's be U, and the set of all P_T 's be V. Then U and V are *isomorphic* if and only if the following conditions are satisfied:

- (1) There is a one-to-one correspondence between U and V, i.e. for each P_R , $f^{-1}g P_R g^{-1}f(x)$, which is P_T , is uniquely determined, and no two P_R 's correspond to a same P_T . And for each and every P_T there is at least one P_R corresponding to it.
- (2) For any two arbitrary elements P_{Ri} and P_{Rj} of U, let

$$P_{Ti} = f^{-1}g P_{Ri} g^{-1}f, P_{Tj} = f^{-1}g P_{Rj} g^{-1}f$$

$$\text{Then } P_{Tj} P_{Ti}(x) = f^{-1}g P_{Rj} P_{Ri} g^{-1}f(x),$$

and similarly for higher combinations also. Note that the second

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condition follows from the first if the associativity holds between f , g , P_R , and P_T . But since associativity is not assumed, the second condition has to be stated explicitly.

The fact that U and V , are isomorphic does not guarantee that U and V , as sets, are the same. If they are, they are said to be *auto-isomorphic*. The fact that U and V are autoisomorphic does not guarantee that each of their elements corresponds to itself. It may be that an element in U corresponds to a different element in V , even though U as a set coincides with V as a set. If each element corresponds to itself, the correspondence between U and V is said to be *element-autoisomorphic*. The concepts of isomorphism and autoisomorphism are discussed in further detail in group theory, ring theory, etc., in mathematics.

The criteria of isomorphism, autoisomorphism, and element-autoisomorphism correspond to increasingly close degrees of similarity between thinking patterns.

In classical epistemology, the individuals' cultural background was either abstract or homogeneous, and hence the encoding and decoding functions, especially their unconscious parts, are assumed to be the same for all individuals concerned. If a sincere will to communicate prevails f_e and g_e^{-1} are usually adjusted so that $g^{-1}f(x)$ will approach x . On the other hand, the adjustment of f_u is usually neglected or avoided by T because it is difficult to notice the characteristics of one's own attitude, behaviour, pattern of life, etc., and even if T notices them, it is difficult for him to change them. One of the advances of communicational epistemology over the classical intersubjective epistemology is the consideration of differences in f_u and f_u^{-1} for different individuals.

Let us now return to the theory of evolving games. In the traditional theory of games, the strategies and their *a priori* probabilities are known and only their actual *a posteriori* choice is an *a priori* unknown. Further, the information, when available, was 'objective' information which did not go through the decoding function different from person to person. (The subjective non-linear evaluation of payment was discussed in some aspects of game theory, but this is different from the decoding of information.) The process of interpersonal communication is a series of tactical moves performed by participants, whose purpose is to increase mutual understanding or to increase reciprocal or unilateral misunderstanding (in deceptive cases). Consequently interpersonal communication is a non-zero sum game. The present theory of games differs from the traditional theory in two respects.

First, the strategies are neither chosen prior to the beginning of the play, nor are they distributed with probabilities; at the beginning of play, a player knows neither his own strategies nor those of the other players, but learns his own as well as the others' strategies as the play proceeds. Secondly, the information available is not 'objective'; it is coded by the transmitter and decoded by the receiver. At the beginning of play, a player knows neither the encoding and decoding functions of the other players, nor the unconscious parts of his own. Furthermore, even much of the conscious part of his own coding and decoding functions, because it is so natural, basic and automatic, escapes his explicit understanding. One does not realize *how* he thinks, encodes and decodes, until he communicates with someone who does otherwise. Thus a player learns a great part of his own encoding and decoding functions as well as those of the other players as the game proceeds. Even if good will for mutual understanding prevails, a large part of a player's encoding and decoding functions remain unknown and incommunicable to other players.

Because this type of game involves learning of strategies, encoding and decoding functions during the play, we call this type an 'evolving game'.

It will be helpful now to introduce the terms *dimension reduction* and *projection*. Dimension reduction is a process by which the interpretation *reduces* the dimensionality of the phenomena interpreted, while projection is a process by which an interpretation *adds* dimensions to the phenomena interpreted. Whether the reduced or projected interpretation is consistent (i.e. without contradiction) within itself depends on the nature of the dimension reduction and projection. A few examples of dimension reduction follow.

Example 1. Consider the motion of a particle moving in a Cartesian co-ordinate system so that its co-ordinates at time t are $x = at$, $y = -t^2 + bt$. The path of the particle is the parabola $y = -(1/a^2)x^2 + (b/a)x$. This is the path of a particle moving in a field of constant force whose vector is $(0, -2m)$ where m is the mass of the particle. This situation is consistent with classical two-dimensional mechanics. The G-projection (using this term to distinguish geometrical projection from projection as defined above) of the motion on the x -axis from the point $(0, \infty)$ gives a one-dimensional motion, $x = at$. This is consistent for classical one-dimensional mechanics. The G-projection of the motion on the y -axis from the point $(\infty, 0)$ gives the one-dimensional motion $y = -t^2 + bt$. This also is

consistent for classical one-dimensional mechanics in a field of constant force.

Example 2. Again consider a two-dimensional Cartesian coordinate system in which two circular particles P_1 and P_2 of equal mass and equal radius r are in motion, P_1 moving along the line $y = a$ with velocity v_x , P_2 moving along the line $y = a - \sqrt{2}r$ with velocity $-v_x$, in such a way that they collide. Let us assume that there is no loss of kinetic energy in the impact. After the collision P_1 continues its motion along the line $x = b$ with velocity v_y , where $|v_y| = |v_x|$, and P_2 continues its motion along the line $x = b + \sqrt{2}r$ with velocity $-v_y$. The whole situation conforms with classical mechanics. If we now G-project the motion perpendicularly to the x -axis, the law of conservation of energy is violated in the resulting picture. After the collision there is no motion. We have to rationalise the anomaly by assuming, for example, potential energy at the points $(b, 0)$ and $(b + \sqrt{2}r, 0)$. On the other hand, a G-projection perpendicular to the y -axis will result in the apparent creation of kinetic energy out of no motion. Again a rationalisation becomes necessary, by assuming, for example, potential energy at the points $(0, a)$ and $(0, a - \sqrt{2}r)$. But a G-projection perpendicular to the line $y = -x + c$ will save the law of conservation of energy.

Example 3. An agnostic has no religious dimension. A philosophical system which includes all his points of view plus a religious dimension may become, depending on the method of dimension reduction, a self-consistent interpretation into an agnostic philosophy.

From a formal point of view the agnostic philosophy is more general than the religious philosophy, because it is less specific. But from the point of view of content, the religious philosophy is more general because it embraces more. The interpretation of generality here depends on whether one considers the addition of an extra dimension as an increase of specificity or as an enlargement.

Example 4. A person from a culture with little interest in the taste of interior decoration visited another culture in which tastefulness was reflected even in the interior of railway trains. Lacking the dimension 'tastefulness in interior decoration', he exclaimed that the train was 'super-clean'.

In contrast to dimension reduction, projection (to be distinguished from G-projection) adds, in the interpretation, extra dimensions which do not exist in the phenomena to be interpreted. Usually the dimensions projected are traits that the decoder himself possesses or traits

that he found in other persons in his previous experience. Projection of traits found in others is, in popular terms, usually spoken of as 'generalisation', but this would be misleading in the present context, and it is better to call it 'extrapolation'. Among the traits most frequently projected are fear, suspiciousness, aggressiveness, connotativity of communication, i.e. communication by hint and insinuation, etc. A suspicious person believes that other persons are also suspicious. An aggressive person expects every one else to be aggressive. A person who uses indirect insinuations reads non-existent meanings between the lines of another person's statement.

A Western philosopher may apply dimension reduction or projection in his interpretation of Chinese philosophy which has no concept of 'substance' corresponding to that in Western philosophy and in which the subject-predicate relation is dispensable. He may insist that there is a word in Chinese which translates 'substance' and that he can identify subject-predicate relationships in the syntax of Chinese. But, from the Chinese point of view, these would be artificial constructs. There would be a dimension reduction if the event-centredness of Chinese epistemology were not recognised. There would be a projection in the sense that the extra dimensions of substance and of subject-predicate relationships are added. It has been emphasised by B. L. Whorf¹ that what is decisive is not artificial possibilities of certain categories, concepts and relationships in a thought pattern, but their naturalness in it. (Whorf has often been misinterpreted to mean that the linguistic structure *determines* the thought pattern; he among several other writers meant only that the linguistic structure influences and in some cases limits the thought pattern.² I have elsewhere discussed the distinction between total denial and partial denial, a distinction always made in Chinese, often absent in English, and always absent in Japanese, as well as imprecise adverbially modifying in English.³ I have also discussed differences in thought patterns between Swedish and Danish cultures in spite of close similarity of linguistic structures.⁴

¹ B. L. Whorf, *Language, Thought and Reality*, 1956, pp. 59, 63, 64, 145, 146, 157, 213, 265, 266

² *Memoirs of American Anthropological Association*, 1954, 79; L. S. Feuer, 'Sociological Aspects of Relation between Language and Philosophy', *Philosophy of Science*, 1953, 20, 85-100

³ M. Maruyama, *Epistemology of Intercultural Understanding*, pp. 147, 148, 151

⁴ *Ibid.*, pp. 75-86, 99-101, 136-144

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Thus, by dimension reduction and projection, a Western philosopher may construct a Chinese epistemology with the concept of substance and with the subject-predicate relationship, which is consistent not only in itself but also with Western philosophy.

We may also remark here that superstition and suspicion, which are variations of projection, are usually self-consistent. They are also self-proving in a circular reasoning. The belief in a supernatural power (an *a priori* assumption) is 'proved' by interpreting, in terms of this *a priori* assumption, certain events to have been caused by a supernatural power. The hostility of a person (an *a priori* assumption) is 'proved' by interpreting, by virtue of this *a priori* assumption, his behaviour to be indicative of his hostility. The 'logical' proof of the concept of substance may be due to the fact that our logic is based on that concept. A philosophy without the concept of substance may as well be self-consistent. The development of logic and mathematical logic in the twentieth century has made it clear theoretically that, even within the deductive framework, it is possible to construct theories, each of which is consistent within itself, but which are incompatible with one another. This theoretical insight is not always appreciated when we reject certain patterns of thinking as 'illogical' or 'unphilosophical'. In fact, such rejections often have 'illogical' bases.¹

We have seen that inconsistency in interpretation indicates one or both of two possibilities: (1) That the interpretation is incorrect, (2) That the interpreter has to change his thinking pattern within which his interpretation appears to be inconsistent. We have also seen that self-consistency of interpretation does not guarantee the correctness of the interpretation. We have discussed some methods of checking for correctness for practical purposes, for example by asking the receiver of the message to process the decoded message and feed it back. We also touched upon the concepts of isomorphism, auto-isomorphism and element-isomorphism of the thinking processes. As for the correctness of another person's interpretation, one has to interpret it in one's own pattern of thinking, and thus the problem becomes that of the correctness of one's own interpretation of others.

There remain yet some questions to be answered: What methods do people use to remove the detected misunderstandings? Are the methods successful? Do they use the best methods? What are the successful and the best methods? The answer to the last question has

¹ L. S. Feuer, 'The Bearing of Psychoanalysis upon Philosophy', *Philosophy and Phenomenological Research*, 1959, 19, 323-340

not been worked out. It needs further studies. The answers to the first three questions are often discouraging. People may accuse each other of being illogical, dishonest, superficial, etc., or bury the misunderstandings under 'politeness', calling this action 'tolerance' or 'considerateness' or even 'understanding'; they may reduce the communication to a practical business level, or may even disrupt communication entirely. In addition to logical factors, psychological factors such as social perception, fear of loss of prestige through failure to get one's values appreciated, 'sour grapes', rationalisation, filtering of perception and memory for reduction of cognitive dissonance, transference, instrumentalising perception, etc., have already been discussed elsewhere.¹ Let us now consider the third stage of our study—the limitations of interpersonal understanding.

(to be concluded)

¹ See Part I of this paper, second reference.

DISCUSSIONS

THE PARADOX OF CONFIRMATION (II)

THIS note is a continuation of a previous one¹ and consists of a correction and an addition to it. The correction is a serious one, and I must apologise for my previous error; but, as extenuating circumstances, I think I was right in applying semi-quantitative methods to the problem. Also I still believe that the idea of a Statistician's Stoooge is useful.

Hempel's paradox is exemplified thus:

The hypothesis, H , that all crows are black is the same as that all non-black things are non-crows, and this is supported by the observation of a white shoe.

The kernel of my argument was to consider a 2 by 2 contingency table with its rows labelled 'crow' and 'shoe', and its columns 'black' and 'white', and then to find an algebraic expression for the weight of evidence in favour of H in virtue of the evidence, E , of the observation of a white shoe. (The use of the Stoooge allows us to restrict our attention to 2 by 2 contingency tables.)

If the marginal totals of this table are known in advance, then E really does support H . But I stated in Part I that this conclusion follows even if the marginal totals are not known in advance, but only have probability distributions. Here I made a mistake.

In order to see this, let us suppose that we know in advance the total number of black shoes and the total number of white shoes that may be observed, each shoe being equally likely to be observed. Then it is a very simple matter to prove that the weight of evidence in favour of H provided by E is zero, where 'weight of evidence' is defined, for example, in Part I.

Thus a 'case of a hypothesis' does not necessarily support the hypothesis. (The example of a white raven shows that a case of a hypothesis can undermine it, but this example is irrelevant to Stoooge-type experiments.)

The question arises, what makes us imagine that a case of a hypothesis must support it? Light is shed on this question by supposing an object to come gradually into view. Suppose that at some moment the object is seen to be white (event F). Then $P(E|F.H) = 1$, where E is the proposition that the object is a non-crow. But $P(E|F.\bar{H}) < 1$. Hence $W(H:E|F) > 0$. For example, the observation of a white shoe supports H when the object is already known to be white. But $W(H:F) < 0$ under a wide class of assumptions, including those where one knows in advance the physical probability that the first object observed will be a black crow. (For on this assumption, the probability of seeing a white object is a little smaller given H than given \bar{H} . Thus it may easily happen that $W(H:E.F)$, which is equal to $W(H:F) + W(H:E|\bar{F})$, may vanish although $W(H:E|F) > 0$.)

Let us express the matter more generally. If we wish to test a hypothesis of the form that all A 's are B 's, we may go on sampling objects until we find an A , and then look to see if it is a B , the observation being of Stooagian type. If it is a B , then the

¹ I. J. Good, this *Journal*, 1960, II, 145-149

hypothesis is to some extent supported *if the evidence of how long it took to find an A is suppressed*.¹ More formally, the weight of evidence in favour of the hypothesis provided by *B*, given *A*, is positive. But what may be overlooked is that if *A*'s are surprisingly easy to find, this may by itself undermine the hypothesis that all *A*'s are *B*'s. Thus the total evidence may not support the hypothesis after all.

This general thesis has already been exemplified above, with *A* = non-black, *B* = non-crow; but perhaps a further example will clarify the thesis further.

Suppose we are told that all men in Ealing whose surnames end with the letter *z* are escaped convicts. We take a random sample of the citizens of Ealing, and, after a very short time, we find one whose surname ends with *z*. Then the fact that we found such a one so quickly tends to undermine the hypothesis, for this evidence by itself suggests that there are more people whose surnames end with *z* than we had previously supposed.

Thus there is more than meets the eye in the expression 'a case of a hypothesis'. In one interpretation it necessarily supports the hypothesis, in another it need not do so.

Although it is not strictly necessary for our argument it seems desirable to elucidate the contention that 'all crows are black is the same as that all non-black things are non-crows', especially as this contention was called into question at a recent London Symposium on information theory by D. M. MacKay.² As propositions the two statements are logically equivalent. But they have a different psychological effect on the experimenter. If he is asked to test whether all crows are black he will look for a crow and then decide whether it is black. But if he is asked to test whether all non-black things are non-crows he may look for a non-black object and then decide whether it is a crow. If he engages a Stooge to perform the second experiment, the Stooge may see several black crows before finding a non-black object, and say nothing about the crows in his report. This second method of performing the experiment would be very inefficient as compared with the first method, in the sense that the expected weight of evidence per unit of effort would be much less. An intelligent experimenter, even if he is asked to test the hypothesis that all non-black things are non-crows, will set about looking for crows, or possibly ravens.

Afterthought. The following example shows that, in very special circumstances, the sight of a white shoe can actually undermine the hypothesis that all crows are black. Suppose that a child had seen black crows, black shoes, and no other black objects, and that all the crows and shoes had been black. He now sees a white shoe and he says, 'How surprising! Apparently objects that are supposed to be black can sometimes be white instead.' On the information available to the child this may be a very rational thing for him to say.

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¹ The need for the observation to be of Stoogian type is exemplified by the white raven.

² In the discussion of the paper which I presented. The Proceedings of the Symposium are to be published by Butterworths.

COMMENTS ON 'THEORY OF RESONANCE'

COMMENTS ON DR NINIAN MARSHALL'S 'THEORY OF RESONANCE'

In his paper 'E.S.P. and Memory: a Physical Theory'¹ Dr Ninian Marshall puts forward a set of assertions which he claims constitute a new theory in physics with applications to some problems in psychology, particularly the problem of the so-called Extrasensory Perception phenomena. The paper is an ambitious one containing a main thesis inviting comparison with Newton's formulation of classical Mechanics. (He formulates a 'law' on the explicit analogy of the Newtonian 'Law of Gravitation'.) Unfortunately when one examines his main thesis one finds a lack of precision which rules out any hope of Marshall's being the long sought 'Newton of the Mind'. Furthermore, as I shall show, there is conflict with empirical fact in connection with the only positive prediction which one can immediately draw from this admittedly 'qualitative' and imprecise, though highly pretentious, paper.

Put briefly, Dr Marshall's main thesis is this: there is a feature in the physical world which has hitherto escaped the notice of physicists, and other natural scientists, because its effects are too small to be detected in the realms with which they are concerned. This feature which the author calls 'Resonance' is said to obtain significantly only between 'complex structures' manifesting 'patterns' which have some sort of 'similarity'. When 'resonance' occurs in a significant degree it is said to be responsible for a wide range of effects, in particular some of those ostensible occurrences called ESP. 'Resonance' is said to obey 'laws' which are 'formally' analogous to two of the laws of Newtonian Mechanics, namely the law of gravitation, and the law of force expressing the relation between acceleration and mass. Dr Marshall contends that on the basis of this theory he can make predictions which can be experimentally tested.

It is obvious that the strength of any such a theory, which claims to be scientific, must be determined by that of its basic tenets. If these bases cannot match the requirements of scientific method in the other sciences which it claims to complement, the theory (and any consequences entailed by it) becomes valueless. This I believe to be the position in regard to Dr Marshall's exposition of his theory, for the reasons that I shall now give.

Firstly, and fundamentally, the defect of the theory in contrast to Newtonian Mechanics is its complete failure to produce precise definitions of the basic concepts of 'complexity', 'pattern', 'similarity' and 'difference' between patterns. Yet these are key concepts in his theory and his so-called 'law of resonance' is expressed in terms of them. The law he states as follows:

Any two structures exert an influence on each other which tends to make them become more alike. The strength of this influence increases with the product of their complexities and decreases with the difference between their patterns.

It will be obvious that the law in this form is useless for the purpose of making any precise predictions of the kind that could be tested. The author seems to think that new mathematical or scientific developments are required to make a more precise formulation possible. This is not so: there are well established methods which could

¹ This *Journal* 1960, 10, 268-286.

perfectly well be used to make his concepts precise, and to make verified predictions from them, if his ideas are sound. For example: the mathematical theory of probability can be used to give precision to the concept of 'complexity' in regard to both structure and function. Indeed it has been so used for some time in the scientific discipline known as 'Information Theory'. The basic idea here is the notion of the antecedent probability of a particular configuration of elements in space (for example distribution of velocities of particles, arrangement of amino-acids along a polypeptide chain, etc.) or of processes in time (for example transitions between energy levels, simultaneous firing of neurones) arising by chance. The greater the antecedent improbability of the actually realised structure or process, the greater its 'information content' and 'information content' certainly corresponds very closely to what Dr Marshall appears to mean by 'complexity of pattern'. Thus the 'information content' of the neural processes underlying a conscious train of thought, such as a scientific discovery, is certainly greater than that of the 'ordering' process involved in the growth of inorganic crystals. Unfortunately the only prediction which seems possible on the basis of the imprecise formulation of the theory by its author, viz. that two almost identical cells should grow more, rather than less alike, is refuted by the known facts of embryology. I say more of this later.

Again the notion of 'pattern' and 'similarity' and 'difference' of pattern, can be made precise by using the theory of relation-numbers set out in *Principia Mathematica* Vol. II, part IV, p. 150 ff. On Whitehead and Russell's theory we start from the notion of structures as consisting of ensembles of entities united by n -adic relations where n is any integer greater than one. Then two relations P, Q are said to be 'similar' if there is a one-one relation between the terms of their fields which is such that, whenever a pair of terms have the relation P their correlative terms, picked out by the one-one relation referred to, have the relation Q ; and vice versa. Two relations which are similar in this sense are then said to have the same 'structure' or 'relation-number'; thus the 'relation-number' of a relation is the same as its 'structure', and is defined as the class of all relations similar to a given relation. Relation-numbers satisfy all the formal laws of arithmetic which are satisfied by transfinite ordinal numbers.

If therefore we interpret Dr Marshall's term 'pattern' to be equivalent to 'structure' as defined in *Principia Mathematica*, we can give further precision to his law of resonance quoted above; though, as I shall now show, there is still uncertainty about the application of the 'law' to his theory of telepathy and memory. In the *Principia* theory of 'structure' or 'pattern' a necessary condition for two ensembles of entities to be 'similar' in structure is that they should consist of the same cardinal number of *relata*, that is the fields of the constitutive relations must contain the same cardinal number of terms. But this condition is not sufficient. For example, the two ensembles consisting of the series of all natural numbers in order of increasing magnitude, and the series of all the odd numbers followed by all the even numbers, have exactly the same cardinal number of *relata*, that is \aleph_0 the cardinal number of the set of all the natural numbers, in each case. But the transfinite ordinal number of the former series is exactly half the transfinite ordinal of the latter. Therefore the two ensembles are NOT similar in structure and do not have the same 'pattern'.

The reason for this is of course that the set of all odd numbers has the transfinite cardinal \aleph_0 of terms, and so does the set of all even numbers. Therefore we can

COMMENTS ON 'THEORY OF RESONANCE'

correlate one-to-one the set of the natural numbers to the set of all odd numbers; and then all the even numbers will remain without a correlate. We can make the two ensembles similar in structure by *replacing* the even numbers in the order in which they occur in the progression in order of magnitude of natural numbers. So the *necessary and sufficient* condition for two ensembles of entities to be *similar in structure* is that it must be logically possible for their terms to be correlated *without change of order*. We can now see that *dissimilarity* between patterns can take two forms:

(1) The two structures compared may *not* have the same cardinal number of related terms. We might call this Cardinal Dissimilarity to be measured by the difference in the cardinal number of the members of the two.

(2) The two structures compared may have the same *cardinal* number of terms but their *ordinal* or 'relation' numbers may be different. In such cases, of what we may term 'ordinal dissimilarity', we can measure the degree of dissimilarity by methods such as Kendall's 'Rank Correlation Coefficient' and his 'Coefficient of Concordance', (to deal with the case of sets of dimensionally independent rankings). In point of fact the easier thing to measure is 'degree of *similarity*' by Kendall's technique. Such degree of similarity will in general take two forms:

(1) degree of direct correspondence between the ordinal numbers of two classes of characteristics, for example rankings in respect of skill in Music and Mathematics in a single school form, that is between two classes having the same *cardinal* number; this degree of correspondence can be measured in terms of the minimum number of moves which transform any ranking into any other ranking of the same collection of objects, for example boys of a given form in a school.

(2) Concordance between the communality of orderings of n objects (e.g. boys or geometrical points) in a manifold of m dimensions (e.g. different observers or m spatial dimensions). The degree of such concordance can be measured by Kendall's Coefficient of Concordance. (*Rank Correlation Methods* by M. G. Kendall, London, 1948, p. 81).

But this brings out the practical uselessness of Dr Marshall's concept of 'resonance' once one gives it precision: how could one hope to establish with experimental techniques the exact *cardinal* number of neurones involved in a particular pattern of neural activity? Even more difficult would be the determination of the precise *ordinal* relationships. Yet both these kinds of data would be required to establish degrees of dissimilarity by reliable methods, for example by using Kendall's Rank Correlation Coefficient (which is a measure of the minimum number of changes of order required to transform or assimilate one structure to another, both structures having the same cardinal number of terms).

Finally I would like to point out that Dr Marshall's theory of Resonance seems to be contradicted by the facts of cell differentiation in embryology. The first stage of normal embryological development is the division of a fertilised ovum into two 'daughter cells' which are practically identically similar in structure to each other and to the single parent cell from which they are derived. At the next stage of embryonic growth each of these two daughter cells produces two others yielding four in all. There is now slightly more difference between each member of the ensemble, but they are still practically identically similar in structure judged by current observational

techniques. As the process of cell division and embryonic differentiation goes on we reach assemblies of cells, the members of which are more and more unlike in structure and function: till finally the full specialisation characteristic of the adult organism is reached. But it is clear that this process of differentiation and cell specialisation as we know it, is contrary to Dr Marshall's theory of Resonance, which entails that the two very closely similar cells, which are 'daughters' of the original fertilised ovum, should grow *more* alike rather than give rise to more unlike 'daughter' cells of their own. It is no use to say that the cells involved are not sufficiently 'complex' structures in Dr Marshall's particular sense to serve as examples of the theory of resonance. For it is a fact that all the vast complexity of the hereditary structure of the human brain is contained in the genetic material of the original cell—the fertilised ovum, and the human brain is, according to Dr Marshall, the most complex structure in the universe.

So I conclude that Dr Marshall's theory of resonance is both theoretically unsatisfactory (because its formulation is so imprecise) and practically invalid (because such conclusions as one can draw from so imprecise a theory are in conflict with well-established empirical facts). It seems clear that ESP phenomena, if they do occur indeed, cannot be clarified by this kind of speculation. Nor can we expect Dr Marshall's theory to be of any help in the fundamental problems of Physics, Biology, and Psychology.

H. A. C. DOBBS

REPLY TO DR H. A. C. DOBBS

DR DOBBS's note on my article 'ESP and Memory: a Physical Theory' offers three main criticisms of the theory of resonance—that it is imprecise, hard to apply, and in conflict with the facts of embryology. I regard one of these criticisms as true, one as over-simplified, and one as completely mistaken.

Dobbs is right to point out that the theory as presented there is imprecise. (I take it that he refers only to that article. For although he calls the imprecision a fundamental defect of the theory, he goes on to say that it could quite easily be remedied by established methods.) I was aware at the time that the basic concepts of 'complexity' and 'similarity' were not sharply defined. To publicise an as yet undeveloped theory is often not of immediate practical use, but may be of value in stimulating thought and discussion on a subject which is too big for the author to handle alone. I appreciate that Dobbs has contributed something in this way. I too shall not be satisfied with the theory until it is expressed more precisely. Indeed, to some extent this had already happened. There is a steadily growing family of possible measures of complexity and similarity, about which I hope to write more later. Here I shall confine myself to the material published so far.

Dobbs's suggestion that one can identify complexity with information content, and so with antecedent improbability, will not do. It is true that some simple structures (e.g. crystals) are more likely to occur than some complex ones, but it is not true in general. Let a line of n pennies show heads and tails at random. Then all the 2^n combinations are equally probable, but they are not all equally simple. (The simplest combinations show all heads, or alternating heads and tails, or the like.) The prior probability of an event depends on the context in which the event occurs,

whereas its complexity is an intrinsic property; the two cannot therefore be equated.

A more adequate approach would have been as follows. Let a standard communication language be defined, containing various algebraic and logical symbols. Let the complexity of a pattern be defined as the length (in this language) of the shortest message needed to describe the pattern. If a pattern is repetitious (e.g. a wall-paper or a crowd) it will pay first to describe code symbols for the repeating units. The total length of the message will then be the length of the description of the code plus the length of the encoded message. Simple structures will tend to have short descriptions. This can be made as precise as one likes, and is one reasonable measure of complexity.

To define similarity is not so easy as Dobbs suggests. He invokes the mathematics of relation-numbers and of rank correlation coefficients, but he does not make clear how these are to be applied. There are several difficulties. His relation-number approach applies in its present form only to structures having equal numbers of identical elements. But structures may have similarities at different levels and of different kinds. For example, a mouse and an elephant have a similar anatomical structure. The grossest similarity is the one-one correspondence between the organs and their relationships in each animal. But the individual organs are themselves structures having varying degrees of similarity. At some levels there are differences not in the relationships between but in the number of elements in the two structures (e.g. a mouse liver has fewer cells than an elephant liver). How can this hierarchy of similarities be used to yield one scalar number, a measure of the degree of similarity of the whole? Dobbs does not say.

I am puzzled by Dobbs's suggestion that we can measure the relationships of points in space by rank correlation coefficients. Rank correlation coefficients measure ordinal similarities. Ordinal relationships are topological relationships in one dimension. How can they be applied to spatial relationships in three or four dimensions? If sets of objects (i.e. structures) are ranked in each of m spatial dimensions, as Dobbs suggests, then unfortunately the coefficient of concordance will depend on the initial choice of co-ordinate axes; it is not an objective measure. This idea may possibly bear fruit, but seems untenable in its present form.

Nevertheless, let us here assume (as I hope to show elsewhere) that a sensible measure of degree of dissimilarity can be given which depends only on the topological properties of the two structures. Two topologically identical but metrically different properties would, in this measure, have zero dissimilarity. But other measures could as well be defined which depended only on the projective, affine, or metrical properties of the two structures. Which type of measure should be used, if the theory of resonance is true, is another problem which has not yet been considered.

Dobbs claims that the theory of resonance is hard to use in psychology because so little is known about brain structure and function. This, of course, is an objection to every theory correlating brain activity with mental processes. With present techniques, no central neurophysiological process can be recorded in any detail. I did not claim that the theory would replace the study of neuroanatomy and neurophysiology; I claimed that it might be true. It should be tested on a complex artefact whose structure is known. In fact, this is feasible with existing electronic structures. If shown thus to be true, the theory would help the study of the brain in two ways. It would suggest some fruitful lines of research which would otherwise be ignored.

And it would provide a further method of recording brain activity—that is building a device which resonated with a living brain.

Finally, I do not agree that the theory of resonance is in any way contradicted by the facts of cell differentiation in biology. Let us suppose that at the two-celled stage an organism is completely symmetrical, but that asymmetry appears as it grows. This paradoxical process requires explanation whether or not the theory of resonance be true. The following explanation has been put forward by cyberneticists. Many structures have this property—for example a pin balanced on its point, or an infinity of electric circuits. These systems are initially symmetrical in structure, but soon acquire an asymmetry of function. For all their states of stable equilibrium are asymmetrical. They can acquire asymmetry of function in any of a symmetrical set of ways. (This fits the facts of equipotentiality of cells in early embryos.) If such a system is also a self-organising system, the asymmetry of function will then lead to the development of asymmetries of structure. Some such asymmetry producing mechanism must exist within a developing organism.

What difference would a resonance influence make to a system of this kind? It would often make very little difference. Resonances would be influences tending to keep the two cells similar, while some ordinary physio-chemical influences tended to make them become unlike each other. Which would win would depend on the relative strengths of the two sets of influences. The occurrence of differentiation would certainly not prove that no resonance influence had acted. Resonance was put forward, not as an all-embracing philosophical theory describing the overt behaviour of any two structures whatever, but as one influence at work among many. This was the point of comparing it to Newton's Law of Gravitation. (Gravitation does not imply that any two touching objects will remain in contact, nor that a meson cannot split.) Dobbs calls the present theory pretentious; to what does it pretend?

NINIAN MARSHALL

A POINT OF PROFESSOR DINGLE'S

REGARDING Professor Herbert Dingle's article 'The Doppler Effect and the Foundations of Physics II', I would like to comment as follows:

In the discussion of the hypothetical experiment of two observers A and Z, firing rockets in the same direction $Z \rightarrow A$, Professor Dingle is in error. dV is not the velocity with respect to something, but the *change* in velocity of A or Z. A change in velocity can be measured, and even a change in relative position! It is the principle of today's inertial navigation, which is a very accurate method and which in no way contradicts the theory of relativity.

It is interesting to note that in the mean time this experiment is no longer hypothetical. It has been done, and it came out exactly as the physicists expected it would. Its result was communicated by H. E. Bömmel and K. Dransfeld at the Thanksgiving Meeting of the American Physical Society (25 Nov. 1960). A difference between the theory of relativity and a classical theory could not be detected in this experiment, since it would be a second order effect, proportional to $(dV/c)^2$. But Professor Dingle's discussion is only concerned with a first order effect. This can be derived either by a ballistic theory or the theory of relativity with identical results.

P. J. VAN HEERDEN

REPLY TO DR VAN HEERDEN

A REPLY TO DR VAN HEERDEN

I CANNOT, of course, in the absence of a description, comment on the experiment to which Dr van Heerden refers, but since he implies the relevance of a second order effect I must presume that, if it is actually my A/Z experiment, a spectrum displacement was observed by A or Z, and in that case an effect depending on motion has been observed in circumstances in which there is no *relative* motion. The effect must reveal absolute motion, and so violate Einstein's first postulate.

But I doubt whether the experiment is really equivalent to mine, for I was not concerned with what happened during the almost instantaneous firing of the rockets, but with a slightly later state of affairs. Suppose A and Z are a light-year apart, and the rocket-firing lasts a fraction of a second. What does Z observe a month later? (The times, of course, could be reduced for a terrestrial experiment, so long as they keep similar relative magnitudes.) If he observes a steady spectrum displacement from which a dV can be calculated, then that dV must on any theory, classical or relativistic, denote a 'velocity with respect to something'; it cannot be called a 'change' of a non-detectable velocity.

Dr van Heerden's remark that 'a change in relative position' can be measured suggests that he has not perfectly grasped the situation. Of course a change in *relative* position can be measured, and so can a change in *relative* velocity. But here, as I pointed out, the *relative* velocity of A and Z is always zero, so if there is a change of velocity it must be of *absolute* velocity.

I might add—though this is outside the subject of my paper—that if, in Bömmel and Dransfeld's experiment, an effect was observed corresponding to the momentary acceleration of the bodies, then, if those bodies had never been in *relative* motion, the general postulate of relativity was violated. In that case, if the special postulate still holds, something meaningless can meaningfully change. It was Einstein's conviction, on both epistemological and physical grounds, that this is inadmissible that led to his general theory of relativity and the consequent theory of gravitation.¹ I suspect, therefore, that if Bömmel and Dransfeld's experiment was really the equivalent of mine, the effect observed must have been due to the forces applied to the bodies, and not to the motion resulting from the application of those forces.

HERBERT DINGLE

¹ *Ann. d. Phys.*, 1916, 49, 769

REVIEWS

Essays in the Philosophy of Science. By Charles S. Peirce.

Edited with an Introduction by Vincent Tomas.

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Values in a Universe of Chance, Selected Writings by Charles S. Peirce. Edited with an Introduction and Notes by Philip P. Wiener.

Stanford University Press. London: Oxford University Press.

Pp. xxv + 446. 31s. 6d.

PROFESSOR TOMAS'S collection of the essays, articles and lectures of Charles S. Peirce pertaining to the philosophy of science is a valuable introduction to the thought of this brilliant and difficult philosopher. By now, thanks to the appearance of several volumes attempting to explain the substance of Peirce's thought and his historical relationship to the pragmatists, a new interest has been generated in his work. It is probably very natural that much of this interest has been focussed on his connections with the pragmatists, i.e. James's too-generous claim of discipleship and the master's good-humoured and gentle disclaimer made a natural starting point for scholarly investigation. Even though the results of such studies have amply demonstrated Peirce's independence of what are usually considered to be pragmatist doctrines, they have had the disadvantage of introducing Peirce to the contemporary philosophical reader in a negative way. First the contrast with James must be established, and then the positive discussion of Peirce's own views may be begun. This has had the disadvantage of making Peirce appear a philosopher who had more original or more sophisticated or more powerful arguments to offer in answer to the same questions that James asked.

It is to be hoped that those who begin their study of Peirce by way of Professor Tomas's collection of writings in the philosophy of science will be prompted to ask some new and penetrating questions. Peirce was a philosopher in the great tradition: a man of science thoroughly at home in the mathematics, physics, chemistry and biology of his day and at the same time filled with a deep and critical understanding of his philosophical predecessors. He did not ask how it was that science was possible; he accepted its reality and was concerned to analyse rather how man, as scientist, conducted his inquiries into the real world. After a brief introduction the reader is presented with a collection of the most important of Peirce's writings in the

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philosophy of science (all originally published in the *Collected Papers of Charles Sanders Peirce*). Included are discussions of the nature of belief and inquiry, the logic of deduction and induction, the rôle of hypothesis in argument, a criticism of determinism, and arguments for the reality of natural laws. Professor Tomas has done us a great service in selecting these essays from the voluminous *Collected Papers* thus making these important writings easily available.

Professor Wiener's book is also a collection of Peirce's writings. While there is a slight overlap with the selections in Professor Tomas's book, at least three-quarters of the material included is completely independent. In addition Professor Wiener has the merit of presenting some important writings not available in the *Collected Papers*: the Lowell Lectures on the History of Science, Hume on the Laws of Nature, and Letters to Lady Welby being particularly interesting.

As its title would indicate, Professor Wiener's book is more concerned with the cultural and humanistic aspects of Peirce's philosophy. It is divided into sections each of which presents the relation of science to some humanistic concern, namely, science, materialism, and idealism; pragmatism, a philosophy of science; lessons from the history of scientific thought; science and education; science and religion. Although the selections are uneven in importance, the total gives an attractive picture of Peirce as a wise and deeply humane man. For him there is no antithesis between science (and a philosophy that is primarily a philosophy of science), and values. Very important in his philosophy is the denial of the mechanistic, deterministic world of the positivists; his is a world of chance that allows full scope for all human aspirations. His tone is always one of rationality, liberalism, and good humour, completely devoid of the sentimental and the platitudinous.

NANCY SUTTON SABRA

The Idea of a Social Science. By Peter Winch.

Routledge & Kegan Paul, London, 1958. Pp. 143. 12s. 6d.

To what extent do, or (since there is little agreement on what they are) should, the methods of the social sciences approximate to those of the natural sciences? The problem is a hoary one and because it is both vague and (therefore?) difficult, debate about it rages still. Mr Winch's contribution, one of the newest, is unequivocally antinaturalistic. For *what Winch proposes to do is to subsume social science under 'philosophy' as it is conceived of by disciples of the later Wittgenstein!* (Almost any formulation of this Linguistic Philosophy, as we may call it, is denounced as misleading. It might be described

as the view that it is a very important task of philosophy to scrutinise the language of philosophers and seek out their misuses of ordinary language and over-burdening of terms. Such discussion of nuances of usage is thought to dissolve many traditional philosophical problems. E. Gellner, in *Words and Things*, provides a well-documented dossier on the school.) In support of this startling position Winch adduces what seem like, *prima facie* at least, new arguments.

First, unlike all other intellectual enquiries which *are* human activities, social science is also *about* human activities. It is the activity concerned with activities. Winch argues that human social behaviour is only intelligible as (or because it is) rule-following behaviour, and that the notion of what it is to follow a rule is fundamental to social science. But the analysis of following a rule is also fundamental in philosophy, according to those who agree with Wittgenstein's emphasis on language. We follow rules in every human activity, even natural science. Only social science, however, tries to be at the same time the science of rule-following while itself, *qua* activity, following rules. And, we are told, Wittgenstein's analysis of following a rule *a propos* of language is applicable to all rule-following activities, i.e. to all human activities, and is thus a fundamental insight of social 'science'. Following-rules-in-society replaces physical-law-obeying-behaviour in Winch's social science. But this analysis of Wittgenstein's shows rule-following to be crucially different from physical law-obeying.

the notion of human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences (p. 72). . . Whereas the man learns to understand the rule the dog just learns to react in a certain way . . . the concept of understanding is rooted in a social context in which the dog does not participate as does the man (p. 74).

Winch's second argument derives from his view of philosophy as 'the study of the nature of our understanding of reality' (p. 40). Philosophy of science, philosophy of religion, political philosophy, etc., are not to be understood as parasitic developments on the edges of science, religion, politics, etc. Rather are they attempts to find out what kind of understanding of reality is conveyed by science, religion, politics, etc. But there is a snag in the way of extending this view to the philosophy of the social sciences for:

to understand the nature of social phenomena in general, to understand, that is, what is involved in a 'form of life', has been shown to be precisely the aim of epistemology (p. 42).

Thus social science *is* the philosophy of social science, which *is*, in turn, Linguistic Philosophy. This state of affairs comes about because the sociological concepts with which we try to render social phenomena intelligible are themselves part of social phenomena.

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The idea of war, for instance . . . provides the criterion of what is appropriate in the behaviour of the members of the conflicting societies. Because my country is at war there are certain things which I must and certain things which I must not do. My behaviour is governed by my concept of myself as a member of a belligerent community. The concept of war belongs *essentially* to my behaviour. But the concept of gravity does not belong essentially to the behaviour of a falling apple in the same way: it belongs rather to the physicists *explanation* of the apple's behaviour (pp. 127-128).

The sociological concept of war influences my behaviour in a way '*essentially*' different from the way the physical concept of gravity influences the falling of the apple (which is not at all). Also the sociologist cannot invent his own explanatory concepts but must work with concepts already used in our social thinking, speaking and acting. For these reasons sociological explanations can never be the same as physical explanations, so much so that Winch does not dub them 'explanations' at all but something more like 'attempts to render human behaviour intelligible'. The difference is, as we shall see, rather important.

Moreover, and finally, he argues that natural science is objective as between the phenomena it studies; its 'view of reality' requires it to be, to use the vogue word, 'ethically neutral'. Nothing, apparently, could be worse for the science of society for:

a . . . sociologist of religion must himself have some religious feeling if he is to make sense of the religious movement he is studying and understand the considerations which govern the lives of its participants. A historian of art must have some aesthetic sense if he is to understand the problems confronting the artists of his period; and without this he will have left out of his account precisely what would have made it a history of *art*, as opposed to a rather puzzling external account of certain motions which certain people have been perceived to go through (p. 88).

Perhaps one's main objection to this book is that this last cited argument leads to the depressing conclusion that social science, like Linguistic Philosophy (which occasionally pretends to be proud of the fact), is impotent. For the very nature of human society, we are told,

is to consist in different and competing ways of life, each offering a different account of the intelligibility of things. To take an uncommitted view of such competing concepts is peculiarly the task of philosophy. It is not its business to award prizes to science, religion or anything else. It is not its business to advocate any *Weltanschauung*. . . . In Wittgenstein's words 'Philosophy leaves everything as it was' (p. 103).

The rest of my comments will be lost on anyone who finds himself in sympathy with this attitude. If criticising and assessing *Weltanschauungen* is not the business of philosophy then I fail to see whose business it is. I

like this sort of activity and when I am doing it I like to think I am doing philosophy; the 'business' Winch wants me to substitute, i.e. 'rendering intelligible' would soon put me to sleep. Problems of intelligibility, communication, 'mental cramps' and so on, are 'the rubbish that lies in the way to knowledge' which we employ an under-labourer to sweep away before we get down to real discussion.¹ For we are interested in pursuing truth, not sweeping away rubbish. We prefer to concentrate on first order problems which it is our task to *solve*. We try to *explain* the social world. 'Understanding' is involved in explanation, certainly, but there is more to it than that: explanation is both more concrete and more objective; which is to say we know what an explanation looks like, and we can usually agree on whether an explanation is satisfactory or not. None of this can we do in the case of 'understanding' and 'rendering intelligible', which both seem to be primarily psychological events.

Winch's arguments about what it is to follow a rule and that the concept of war influences our behaviour, seem to me to be a mere sophisticated re-vamping of the well-known 'oedipus effect' and 'intuitive insight' arguments. But there is one new point. We are told that we must have a religious feeling in order to understand a religious movement. (Do we need it to *explain* a religious movement?) We at once wonder whether this is any more true than the manifestly absurd idea that we have to empathise with an atom in order to understand its behaviour. 'Ah', Winch could reply, 'you are already begging the question; my whole point is that there is an *essential* difference between human behaviour and the behaviour of physical objects. The concept of war belongs essentially to human behaviour.'

What Winch means is that the idea of war influences human behaviour: it is one of the factors in the situation of the individual. While the falling apple has no situation in that sense, and is not influenced by the concept (only by the force) of gravity. But in as much as the force of gravity can be said to dispose the apple to behave in a law-like fashion, i.e. 'falling'; so can the concept of war be said to dispose human beings to act in a law-like fashion. Both 'gravity' and 'war' are dispositionals: the only difference being one of degree, not kind: a human being is disposed, *qua* physical object, to respond to all the forces that the apple responds to; but in addition a person has the power, *qua* decision-taking, free-willed individual, to counter-act some of these forces, and is burdened with the nonphysical influences of other people, motives, concepts, theories, compulsions or whatever. To explain why that boy fell when he let go of the branch we need no more than we

¹ There is a contradiction in Winch's book: a good deal of chapter I is spent denouncing the under-labourer conception of philosophy. Unfortunately the import of the book is not only to endorse Wittgenstein's under-labourer-ism ('rendering intelligible' etc., etc.), but even to recommend that sociologists become this kind of under-labourer too.

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need to explain the fall of the apple. But explanations of why the boy was in the tree and why he let go of the branch would be very different from explanations of why the apple was on the tree and why it 'let go'.

To sum up. Gellner, an outsider to Linguistic Philosophy, first drew attention in *Words and Things* to the very strong sociological strands in Wittgenstein's later thought: in particular his view that words—read 'concepts'—are intelligible only in their particular sociolinguistic context. Winch, an insider, wishes to invert Gellner's thesis that philosophy is poaching on (poor) sociology, in an attempt to convince us that sociology, rightly considered, is the same as Linguistic Philosophy. But if, as may be the case, Gellner has demolished Linguistic Philosophy in that same book, then the final prop under Winch's case for a more *a priori* sociology is pulled away.

I. C. JARVIE

The Logic of Social Enquiry. By Quentin Gibson.

Routledge & Kegan Paul, London, 1960. Pp. x + 214. 24s.

MR GIBSON's main problem is the relation between the methods of the natural and the social sciences. In his introduction he rejects antinaturalism. But, although he is antinaturalistic, Mr Gibson is not unequivocally pronaturalistic. As a consequence, the book has an odd structure. Its first part is devoted to answering the objections of the antinaturalists; its second part discusses the 'logical peculiarities' of social enquiry.

The *objections* to social 'science' dealt with are: that it is too abstract; that it cannot formulate general laws; that it must use intuitive insight; that it is value-loaded; and that it is nonobjective. Nothing unusual here, you might say, and you would be right. However, it is clearly and succinctly done; although a good deal of it has already been done still better by Popper in *The Poverty of Historicism*.

The *logical peculiarities* of social science which Mr Gibson admits are these. All alleged social facts, and all alleged social laws, are, in the last resort, themselves explicable in terms of the aims and dispositions of individuals and, although this has not been carried out in every case, there seems no reason to believe that it could not be. No hard-and-fast line can be drawn between psychology and sociology: either psychology is equivalent to 'human nature' and therefore includes sociology, or it is the narrow subject taught in universities in which case it is yet another social science for the social environment is one of the factors it must take into account. The use of law and tendency statements in social science is restricted by the very nature of the subject-matter; this is to some extent compensated for by the fact that human beings (unlike atoms) act rationally very often, which simplifies the

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task of explaining their behaviour. The logic of social enquiry closely resembles that of historical enquiry except that historians are interested in the statements of initial conditions and social scientists in the theoretical statements. The book concludes with a chapter pointing out that the implementation of reform based on sociological knowledge is hindered by the necessity of gaining power; an aim which tends to make people lose sight of their original reformist aim.

I must confess to not knowing in which sense any of Mr Gibson's 'logical peculiarities' are either logical or peculiar. Whatever he chooses to label them, though, does not affect one's agreement.

Where do the faults, if any, of the book lie? Principally, I should say, in its inductivism. Not that I now want to rehearse ancient objections to inductivism; although it is about time that people rejected Hume's extremely unsatisfactory solution to the problem, especially as it commits us to a form of irrationalism. More interesting is the mistake Gibson makes which is directly consequent on his inductivism. Believing that we learn by induction from experience Gibson has to admit that questions about the origins of our beliefs may be relevant to their truth. That is to say, one way of checking the truth of our beliefs is to see whether they were arrived at in a sound way. If they are so arrived at then they are 'rational' beliefs in a way in which beliefs about walking under ladders are nonrational (to call them irrational prejudges the issue). Unfortunately I cannot see the difference between these two types of belief. As far as I can see Einstein's theory may have been a hunch of his in exactly the same way that the superstition about walking under ladders was originally someone's hunch. The *theories* are neither rational nor non-rational: they are nothing; the categories do not apply. It is the *belief in* the theories that can be so judged; but even here their origin is and must be irrelevant. It is more rational to believe in some of our hunches rather than in others simply because some of our hunches are better tested than others. Testing, clearly, has nothing to do with origins.

Gibson's view means he must condemn the mythologies and beliefs of primitive tribes as nonrational; while my view both distinguishes our beliefs from those of primitive tribes by pointing out that they are better tested and that therefore our *belief in* them is better founded than their belief in theirs is, and at the same time staves off that total relativism towards beliefs which anthropologists and linguistic philosophers alike have found to be the only resort after giving up the position Gibson takes.

My final criticism is of the book's style. Not, of course, of its literary style, which is clear and easy to read. Rather its exposition or, if you prefer, its method. The book has a dearth of illustration, not just of social laws, which may be difficult to come across, but of problems. The book exists in a vacuum, as it were; no attempt is made to let its argument stem from the treatment of a specific problem or set of problems as have most of the best

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treatises on the methods of science.¹ Mr Gibson wanted, or his publishers asked him, to write a book on the logic of social enquiry; as a consequence the book is, in the bad sense (is there any other sense?), a *text* book and, like most textbooks, rather dull.

I. C. JARVIE

Critical Problems in the History of Science. Edited by Marshall Clagett.

University of Wisconsin Press, Madison, 1959. Pp. xiv + 555. \$5.

THIS volume contains the Proceedings of the Institute for the History of Science held at the University of Wisconsin in September 1957, the primary purpose being to stimulate the study of the history of science. The names of the contributors make up an impressive list, and most of the papers give clear evidence of the tremendous advances in scholarship and insight that have taken place in this field during the past twenty-five years or so. All the major branches of the physical and biological sciences are represented, and the time-scale covered reaches from the Greeks to the nineteenth century. Papers like those of Professor Derek Price's 'Contra-Copernicus', being 'a critical re-estimation of the mathematical planetary theory of Ptolemy, Copernicus, and Kepler', or Professor E. J. Dijksterhuis's balanced survey of the origins of classical mechanics from Aristotle to Newton, will cause many old-fashioned estimates of these crucial periods to be revised. Professor Thomas Kuhn's study on 'Energy Conservation as an Example of Simultaneous Discovery' brings together an immense amount of information and is already recognised as a minor classic in its field, whilst Professor I. Bernard Cohen's 'Conservation and the Concept of Electric Charge' brings to life a subject too often ignored in the standard histories of science. And these are only samples.

For the readers of this journal of course the predominant interest will be philosophical; and it is of interest to note that throughout this volume the relevance of philosophical questions for an up-to-date treatment of historical problems is not lost sight of; indeed two or three papers make this the central issue. Needless to say, the contributors do not speak with one voice. Some, like A. C. Crombie, believe that 'a philosophical interpretation of the nature of modern scientific method and thinking enters into our historical interpretation of the course of events' (p. 80). But whilst for Crombie this is of importance here only because he claims that classical mechanics and optics in part at least had their origin in reflections of the medieval methodologists on the nature of scientific knowledge, Father Joseph Clark believes that the researches of the historian themselves must be

¹ Just one example from social science: Malinowski's functionalism grew out of his grapplings with the problem of the *kula*—or so I conjecture. Only later did he expound the doctrine *in abstracto*.

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carried on not by applying chronological criteria but rather by taking into consideration 'the structure of a logically and systematically relevant pattern of central ideas' belonging to the field of philosophy of science (p. 104). Father Clark thereafter uses his conclusions concerning the logical structure of scientific theory, of the function of mathematics in science, and of the question of the correlation between abstract theory and experimental fact, to illuminate certain historical aspects such as the theory of Copernicus, the mathematical work of Oresme, and Beeckman's and Descartes's theories of free-fall. Apart from the question of the feasibility of this method for the historian, all this of course assumes an extant body of contemporary agreement concerning the nature of scientific theory; an agreement which Professor Ernest Nagel's incisive critical comments are quick to expose as being open to doubt. For Father Clark's views on the 'hypothetico-deductive method', the assumed free and arbitrary assumption of its postulates, and the putative 'isomorphism' between the formal structure and the physical world, are all matters open to debate. In other words, doubtful philosophy has to be used to estimate the relevance of perplexing historical problems.

Still, nothing better could perhaps be expected; rather use provisional philosophical conclusions than none at all. But the matter is even more complex when we find that other contributors plead for the opposing (or is it complementary?) contention that no understanding of the philosophical issues of science will be possible without a grasp of its historical background. Thus, for Dijksterhuis 'the History of Science forms not only the memory of science, but also its epistemological laboratory' (p. 182). To give an instance from Professor Cohen's paper: for an understanding of the place of abstract philosophical maxims such as the causal principle in the body of a scientific theory, it is at least relevant to know that for those early investigators who speculated about the one- or two-fluid electrical theories we can now see that it did not make much 'sense to speak *separately* of the concepts of charge in terms of the electric fluid and the law of conservation of charge' (p. 366, my italics). Again the admitted lack of experimental evidence in favour of either theory, as this fact appeared in the eyes of the eighteenth- and nineteenth-century 'electricians', is of great importance to the philosopher of science when he comes to discuss the concept of 'mere mathematical hypothesis' (p. 370).

Sometimes the historians seem to go even further in their claim of the historical relativism of philosophical theory. Thus, Cohen, in the same paper, believes it to be provable that 'the philosophical point of view of such men as Stallo, Mach, Duhem, Poincaré, and others, is only mistakenly considered in relation to the subsequent development of physics. Their philosophy was much rather a reaction to the problems that had been raised by the science of their own century' (p. 371). I profess that I am puzzled

by this remark. It may of course be true that Mach's views were stimulated by reflections on the nineteenth-century physical scene. But it hardly *follows from this* that Mach's 'philosophical discoveries' (if I may call them thus) were *not* both true, and relevant for the physics of the twentieth century—even if *in fact* they should turn out to have been a muddle.

Professor Kuhn also, at one point, seems to be intent on giving the minimum of credit for the possibility that certain people at a critical stage might have been motivated by perfectly general and not by accidental historical influences. Among the three factors responsible for the conservation of energy principle he rightly singles out one of a 'philosophical' kind which he calls 'philosophy of nature'. As he understands this, however, it means that specific stream in German philosophy at the beginning of the last century going under that name. He then argues (p. 338) that since the 'metaphysical counterpart' of the dynamical conservation theorem must have been next to forgotten during the third decade of the nineteenth century, 'Naturphilosophie' with its emphasis on the convertibility of natural forces must alone have been responsible for the discovery of the conservation principle. However, it should be said that one of its discoverers, J. R. Mayer, certainly disclaimed all association with this philosophy. But this does not stop Professor Kuhn. Though 'Mayer did not study Naturphilosophie . . . , he had close student friends who did' (p. 339).—But why should Mayer not have been under the influence of quite *general* considerations, as he explicitly claimed that he had been, when again and again stressing, both in his writings and his letters, the central importance of the causal principle, expressed as 'cause = effect'? Once one gets a clear appreciation of what are and what are not genuine causes (i.e. forces) in nature, he wrote to Griesinger (30.11.42), then 'the drivel of the nature-philosophers is put in the pillory in all its miserable nakedness' (Weyrauch, ed., *Kleinere Schriften*, 1893, p. 181). In other words Mayer could think for himself. It wasn't just (as Professor Carl Boyer remarks on this point) that Mayer could read, and that there were libraries! (cf. p. 389).

Of course, there is a reason for Boyer's criticism. For he has even less sympathy than Kuhn with the 'philosophical' aspect. When Kuhn refers to this aspect as an attempt to give a 'reasonable justification' (p. 386), Boyer understands this 'to mean some clear-cut mathematical or experimental justification' (ibid.). No wonder he thinks that 'the maxim *causa aequat effectum*' does not 'settle the problem'! He is very suspicious of all this sort of talk, just as he takes Kuhn to task for having declaimed (surely correctly?) that 'energy is conserved; nature behaves that way' (p. 323). This, to Boyer, 'seems to imply a notion of energy as something actually *in* nature which we discover, rather than as a concept which we invent as one appropriate means of describing nature' (p. 385). Shades of Mach! The same Mach whose ideas Professor Cohen seems to be wanting to tame by

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exposing their historical relativity with respect to nineteenth-century physics!

As I suggested in my remark on Father Clark's paper, we are not at all agreed on philosophical matters in science. And there is, as the later papers show, an even greater room for doubtful and tentative solutions, if we realise that historical interpretations play into epistemological solutions, and that at the same time, the former (e.g. of what Mayer did or did not mean or say or do) themselves live under the cloud of the traditional philosophical issues.

There are few books at the present time which will drive home so forcibly these lessons; a fact made particularly easy by the excellent feature of printing the replies to the various papers as well as the originals. Let us hope that there will be many successors.

GERD BUCHDAHL

Fads and Foibles in Modern Sociology and Related Sciences.

By Pitirim A. Sorokin.

Mayflower Publishing Co. and Vision Press, London, 1958, viii + 357.
50s.

PROFESSOR SOROKIN is a very angry old man who now stands disregarded alongside the growing edifice of American social science, in the ground-floor of which he was a master builder. He began writing as a sociologist in Russia before the First World War. Between the wars he was the author of the best known standard work on sociological theories and of a definitive treatise on social mobility which, though it has often been neglected, has yet to be supplanted. Since the Second World War he has increasingly devoted himself to the study of the principles and development of altruistic love and creativity (e.g. *The Ways and Power of Love*, 1954). In this his latest book, however, he turns aside from his more amiable pursuits to fire a violent cannonade into the office and laboratories of the above mentioned building. He offers us, in other words, an armed excursion into the philosophy of science.

The main artillery is directed on to American empiricism in sociology and psychology but other fads 'and foibles including amnesia (especially of Sorokin) and the discoverer's complex, obtuse jargon and sham scientific slang' enable him to define the target widely enough to include most members of the Harvard Department of Social Relations over the last generation and notably Talcott Parsons whose essays on social stratification for example are described as 'largely useless for empirical research' the problems being 'submerged in a sea of ponderous ruminations' (p. 324). English readers, especially of the *Times Literary Supplement*, will doubtless enjoy all this as a rumbustious knock-about turn from the other side of the Atlantic. In fact it is done with much greater economy and elegant wit by Ernest

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Gellner in a review of the 1956 American edition of the book in the current number of *Inquiry*.

Sorokin lambastes the empiricists as quantophrenics and testomaniacs. Those who fear that the scientific skill of the psychometrician will facilitate the rise of the meritocracy will take comfort from Sorokin's view that 'if the current over-evaluation of tests is continued, one of its results will be an increasing misselection and maldistribution of individuals in various social strata and positions'. But the centre of the target consists of slavish imitative worship of that outmoded deity Newtonian macro-physics. (That is why Gellner describes modern American social science as a cargo cult.) The 'cult of numerology' and the 'bootlegging of mathematical formulae', indeed most of the testing, measuring, surveying and experimenting of contemporary sociology are nothing more than the vain repetitions of the heathen. Examples are embarrassingly frequent and familiar and Sorokin exposes them in all their inglorious splendour.

The task of demolition thus energetically completed, the author lays out his plan for re-construction—'the integralist conception of reality, knowledge and ways of cognition'. Here he is much less satisfactory. His greatest emphasis is laid on the importance of the rôle of intuition in science—'the creative intuitional flashes of the great geniuses that . . . reveal to them the essentials of a scientific discovery or of a creative masterpiece, are also of the same nature as the state of *samadhi* of a genuine yogin' (p. 290). But the reader will get a very much clearer and more comprehensive view of the methodological problems of induction, deduction, and theory construction from the works of Karl Popper which, though they constitute the most important treatment of the philosophy of the social sciences to appear in the present century, receive no mention from Sorokin. Popper's discussion of predictability is similarly much superior to Sorokin's, which appears to make prediction a matter of tense and fails to see its logical identity with explanation. Moreover, as Jean Floud has pointed out,¹ no amount of Sorokin's 'superconscious intuition' can ever dispense with the need to test generalisation against fact—a process which involves quantification, testing and experiment.

A. H. HALSEY

The Anatomy of Judgment. By M. L. Johnson Abercrombie.
Hutchinson, London, 1960. Pp. 156. 25s.

THE subtitle to Mrs Abercrombie's book is 'an investigation into the processes of perception and reasoning'. This investigation was carried out on medical students and includes a description of a course that they received

¹ *British Journal of Educational Studies*, 1957, 6

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which was designed to help them to make accurate observations, to draw reasonable influences from and to make critical evaluations of scientific data. During this course it was discovered, especially by the students, that each had his own schemata, or set of expectations which delimited what they saw as well as what they inferred. This discovery by the students was made during skilfully directed group discussions. The discussions were, technically, free discussions in that any member of the group could say what they liked. However, the skill shown by the investigator involved a great awareness of the techniques of group psychotherapy, and it is in this sense that the direction took place. A less aware teacher could have ruined the discussions.

Because the book looks as though it is an account of a new and fruitful teaching method, some readers of this *Journal* might miss the fact that it is an excellent account of the psychology of scientific method. I recommend the book to anyone who still thinks that scientists are completely inductive. The findings are excellent illustrations that the mind is not what Professor Popper has called an empty bucket, and that the basic process in science is the critical evaluation of our own conceptual framework.

From the practical point of view, I am sure that the course given to these students will turn out to be the most important part of their training.

R. F. J. WITHERS

ANNOUNCEMENT

BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Sixth Annual Conference: 22nd-24th September 1961

The Sixth Annual Conference of the British Society for the Philosophy of Science will be held at St Hugh's College, Oxford, from 22nd to 24th September 1961. The programme will be as follows:

'Explanations in Psychology', Dr M. Treisman, Dr J. O. Wisdom.

'Learning Machines', Dr F. George, Professor D. M. MacKay.

'Genetic Coding and Information Theory', Dr I. Leslie, Dr S. Brenner.

'Statements about the Universe', Mr R. Harré, Dr W. Davidson.

Details may be obtained from Dr M. B. Hesse, Whipple Museum, Free School Lane, Cambridge.

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(a) BOOKS RECEIVED FOR REVIEW

- Bacon, F., *The New Organon and Related Writings*, The Liberal Arts Press, New York, 1960, pp. xli + 292, \$1.35
- Berlyne, D. E., *Conflict Arousal and Curiosity*, McGraw-Hill Pub. Co., London, 1960, pp. 350, 58s.
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